CANADIAN JOURNAL OF PSYCHOLOGY

VOL. 11, NO. 3, SEPT., 1957

PUBLISHED FOR THE
CANADIAN PSYCHOLOGICAL ASSOCIATION BY THE
UNIVERSITY OF TORONTO PRESS

Stacks
Replace
Direct
6-5-63
Replace.

CANADIAN JOURNAL OF PSYCHOLOGY

VOLUME 11, NO. 3 SEPTEMBER, 1957

PUBLISHED FOR THE
CANADIAN PSYCHOLOGICAL ASSOCIATION BY THE
UNIVERSITY OF TORONTO PRESS

AUTHORIZED AS SECOND-CLASS MAIL, POST-OFFICE DEPARTMENT, OTTAWA

CANADIAN JOURNAL OF PSYCHOLOGY

Editor: J. D. KETCHUM *Assistant Editor*: H. O. STEER

THE CANADIAN JOURNAL OF PSYCHOLOGY is published quarterly in March, June, September, and December. ***Annual subscription*, $4.00; *single number*, $1.00.**

Subscriptions. Orders and correspondence regarding subscriptions, change of address, and purchase of back numbers should be sent to:

THE SECRETARY TREASURER, CANADIAN PSYCHOLOGICAL ASSOCIATION
Box 121, Postal Station K., Toronto, Ontario

Contributions. Original manuscripts and correspondence on editorial matters should be sent to:

THE EDITOR, CANADIAN JOURNAL OF PSYCHOLOGY
100 St. George Street, Toronto 5, Ont.

Information for contributors. The *Journal* publishes experimental and theoretical articles in all recognized fields of psychology. Contributors need not be members of the CPA or residents of Canada. Manuscripts should be double-spaced throughout, and should follow standard practice as regards tables, references, etc. "Immediate publication" (i.e. in the next issue to go to press) can be arranged for authors willing to pay the extra costs involved.

CANADIAN PSYCHOLOGICAL ASSOCIATION, 1956–1957

President: JULIAN M. BLACKBURN, Kingston; *Past President*: GEORGE A. FERGUSON, Montreal; *President Elect*: W. E. BLATZ, Toronto; *Secretary Treasurer*: R. B. BROMILEY, Toronto.

1957 Annual Meeting
University of Toronto
June 6, 7, and 8

The Canadian Psychological Association also publishes *The Canadian Psychologist*, which is distributed to members only. *Editors*: JEAN GARNEAU and ERNEST POSER, Montreal

GEORGE HUMPHREY[1]

JULIAN BLACKBURN
Queen's University

ONE PURPOSE of our meeting this year in Toronto has been to pay a well-earned tribute to the dean of Canadian psychologists, Professor Bott. I know it will not be misunderstood if in my presidential address this year I pay a tribute to another of the founders of Canadian psychology, a man who also retired from his official position in 1956, and who, in the critical early days of the war, collaborated closely with Professor Bott—George Humphrey of Queen's and Oxford.

George Humphrey was born in 1889. After taking Greats at Oxford he became Professor of Classics at St. Francis Xavier University. From there he went to Wesleyan, where he came under the influence of Raymond Dodge, and in 1924 he came to Queen's. For thirty years before this time the psychology taught at Queen's had been called "Moral Philosophy." The Department of Philosophy had built up an international reputation under Dr. John Watson (not to be confused with John Broadus Watson), a Kantian philosopher who was head of the department for 51 years until he retired in 1924 at the age of 75. Watson had dominated the scene for so long that he had become a rather awesome figure and "it was," as Humphrey has written, "in some trepidation that I came as a young man to succeed him. Fortunately my interests were along different lines from his, and I had been specifically asked by O. D. Skelton, then Dean of Arts, to build up the psychological side of the department."

At first there was no laboratory, but Humphrey begged for a small room in the basement as a kind of primitive working place. Some interesting work had been begun when the building burned down and Humphrey was offered the hospitality of the Department of Biology. It was here that his work on habituation in the snail and on the conditioning effect of pure tones and arpeggios was undertaken—work which helped to establish for Humphrey an international reputation, and work which was the starting point for his impressive book on learning published in 1933.

When the Arts building was rebuilt, one room in the northwest corner was provided with water and other elementary requirements for a psychological laboratory, and soon after R. C. Wallace became Principal of Queen's, steps were taken to provide the psychology staff with a proper laboratory. This was found on the top floor of the Biochemistry

[1]Presidential Address delivered at the Annual Meeting of the Canadian Psychological Association, Toronto, Ontario, June 7, 1957.

CANAD. J. PSYCHOL., 1957, 11 (3)

building, and D. O. Hebb was brought in to develop research with rats. At the outbreak of World War II Humphrey took an active part in organizing the Canadian Psychological Association, and he, perhaps more than any other single person, was responsible for developing the "M" test. In 1947 he left for a year's sabbatical leave in England and Brother Philip took charge of the department. In 1948 Humphrey was appointed to the first chair in experimental psychology at Oxford.

The establishment of this chair had been urged for a long time. Titchener had wanted to become the first experimental psychologist in Oxford when he returned there in 1892 after two years with Wundt, but Oxford had no place for him and he emigrated to Cornell. Eleven years later, in 1903, McDougall was appointed Wilde reader, but since by the terms of the appointment no experimental work in psychology was to be undertaken, McDougall left, disgruntled, first for war work in 1914, and then for Harvard in 1918. As he says in the preface to his *Outline of Abnormal Psychology*, published in 1926, "As regards psychology itself, I obstinately continue to be optimistic. . . . I carry this optimism very far. I anticipate that at no distant date, perhaps before the end of the century, even the University of Oxford may begin to take interest in the human mind and may set her hall-mark upon psychology by giving it a recognised place among her studies." In 1948, however, over fifty years before the end of the century, George Humphrey was appointed as the first professor of experimental psychology in Oxford. At Queen's, Brother Philip held the fort for a further year, and then in 1949 philosophy and psychology were split to become two departments and I was appointed to the chair in psychology.

For the twenty-five years between 1924 and 1949 the usual relationship between psychologists and philosophers was reversed at Queen's, and the Department of Philosophy was under the direction of George Humphrey, a psychologist. Furthermore, the internal conflict between philosophers and psychologists, so usual elsewhere where the psychologists have started life under the leadership of a philosopher, was avoided. George Humphrey himself attributes this in his characteristically modest way to the fact that "the philosophers were such nice people." I think it only fair to add that the principal reason was that George Humphrey himself is such a nice person.

In the United States, where psychology started off with an independent existence of its own, the particular strains with philosophy have been avoided, though in their place there have been strains rather more widely distributed between psychology and most of the other older departments. But Queen's is, I think, unique in that its Philosophy Department was run by a psychologist, and the strains between the two have been noticeable

by their absence. It is an achievement of which Queen's and George Humphrey should be proud.

In his early contributions to the literature Humphrey was concerned with the problem of explaining the new psychology of Pavlov. In a paper which he published in 1920, for example (2), he argued that the Freudian wish was the reaction to a complex system of simultaneously occurring interconnected conditioned reflexes, and that conflict situations arose because some of these constituent reflexes were excited and others not, thus throwing the whole complex into a condition of stress. Thus the discomfort experienced in conditions of conflict, both pathological and "normal," was due to the partial excitation of a system of interconnected reflexes rather than to the arousal of incompatible systems. On looking back at this paper from a distance of nearly forty years one is impressed by the persuasive nature of Humphrey's argumentation and illustrations, but one feels at the same time that he does not provide any clue as to why partial arousal *per se* may lead to such violent reactions as sometimes occur in conflict, especially where the reaction within the whole system of associated reflexes is relatively mild, and especially (as Humphrey himself states) since there is no evidence that mere inhibition does psychological harm any more than, according to Sherrington, it does physical harm. The main purpose of this paper, however, was (1) to take the first steps against the anthropomorphism of Freud—the picturesque descriptions of separate faculties battling to the death within the mind, and (2) to begin the process of linking up apparently very different aspects of psychology—one of Humphrey's most important contributions throughout his career.

The following year Humphrey returned to the attack and in a long article (3) attempted to apply conditioned reflex principles to other Freudian mechanisms such as the complex, compensation, projection, rationalization, sublimation, and transference. Humphrey's explanations were often ingenious, but he was frequently forced to counter Freudian assertions with contrary assertions of his own, no evidence existing at the time for either assertion or counter-assertion.

In 1922, Humphrey extended conditioned reflex principles still further (4) into the field of social psychology. He presented a case for regarding sympathy, "that fellow feeling which we experience when another member of our own species is in physical trouble, pain or some other such elementary situation," as much more automatic than McDougall and others had implied. Thus, "If I hold my hand in a candle, I experience the sensation of pain together with the feeling of unpleasantness. If someone else holds his hand in the flame, I do not experience the sensation of pain but I do have the feeling of unpleasantness, because the sight of

my hand in fire has in the past been accompanied by feelings of unpleasantness" (4, p. 116). One obvious criticism of this theory is that sympathy could be felt only on the observation of situations that we had experienced personally ourselves—unless the generalization factor is made so wide as almost to become meaningless. A further criticism is that even if Humphrey's explanation did apply to this type of sympathy, namely feeling *with*, it does not help to explain the kind of sympathy, namely feeling *for*, in which one person experiences a very different kind of feeling from that experienced by another.

In 1924, however, one notices a turning point in Humphrey's theoretical position, with his publication of two papers on Gestalt psychology. The first (5) draws a parallel between the then new principles of Gestalt psychology as formulated by Wertheimer and by Köhler and the new principle of relativity as formulated by Einstein. Humphrey was clearly much impressed by the new developments. In the second paper (6) he discusses the possible implications of the principles of Gestalt psychology for a theory of learning. (Koffka was, of course, himself engaged at this time in a similar and much more comprehensive task which culminated in his *Growth of the Mind.*)

Hence it is not surprising to find Humphrey, in 1925, beginning to express doubts about the universal validity of the concept of the conditioned reflex as an explanation of behaviour. He sets himself the task of exploring Pavlov's and Watson's and his own earlier view that the conditioned reflex is the unit into which all habits may be resolved. In the paper he published on this subject (7) he was obviously influenced by the work of the Gestalt school, and by Head's still more recent studies in neurology. Humphrey was led to the conclusion that the apparent objectivity of Behaviourism might be misleading—that to break the psychological situation down into the sum of a number of isolated single stimuli, such as single tones and noises, simple shapes, and so on, was a crude procedure and might well account for some of the differences found between the kind of habits established by Pavlov in his laboratory and the kind of habits established outside the laboratory. In other words, the Pavlovian conditioned reflex should be regarded as a special type of habit rather than as the unit into which all other habits may be analysed.

In this paper Humphrey makes an observation that may have been the starting point for his next and perhaps most famous paper. In dealing with the differences between Pavlovian conditioned reflexes and habits, he refers to Pavlov's report that, when a conditioned response had been established to a simple stimulus, the response was inhibited when a compound stimulus containing the original simple stimulus was presented, and points out that this differs from habits in ordinary life. Cor-

respondingly, when a response had been established to a complex stimulus, such as a musical chord, the presentation of one note from the chord produced a smaller secretion of saliva than two or three notes played together.

In his next paper (8) Humphrey puts these two observations together. He established a conditioned response to electric shock following the playing of an individual note. He then found that when this particular note was included in a musical phrase or in an arpeggio the conditioned response did not occur. The influence of the Gestalt school is even more noticeable in this paper than it was in the previous one, and Koffka makes full use of Humphrey's conclusions (14). It is interesting, however, to notice that Humphrey's results were based on the records of only three subjects, and that only the most rudimentary statistical methods were employed.

Humphrey was not the only person to express doubts about conditioned reflexes (of the Pavlovian type) at this time. Within the next two or three years a number of other papers appeared claiming that the laws of conditioning of the Pavlovian type could not reasonably be expected to explain all types of learning, as Pavlov had asserted. This, together with the work that Skinner was beginning to produce, shifted the emphasis from conditioning of the Pavlovian type to instrumental conditioning. Thus Schlosberg (15) showed that the kymographic record obtained from a conditioned knee jerk was qualitatively different from the records of the unconditioned knee jerk; and Upton (17), who conditioned guinea pigs to pure tones of high pitch, and Wever (18), who conditioned cats to tones of high pitch, both found in their records evidence that the conditioned response was qualitatively different from the unconditioned, the former containing in the records of the animals' breathing evidence of an anticipatory reaction which Upton called the "lullaby effect" and Wever the "flutter response." There were other experiments along these lines, but they were directed towards the investigation of the similarity or dissimilarity between the conditioned and the unconditioned response. Humphrey's work was more fundamental, showing as it did that in conditions in which the factors which might be expected to produce external inhibition had been eliminated, the effectiveness of a particular stimulus depended not on the stimulus itself but on its relation to other stimuli presented either simultaneously or successively, and this, of course, was what the Gestalt psychologists had been saying in the field of perception.

We now come to three important papers published by Humphrey in 1930 (9, 10, 11) which foreshadow the principal original contribution he made in his book, *The Nature of Learning*. The first, published in *Psychologische Forschung*, was written before W. B. Cannon's famous

paper on homeostasis (1) had appeared and is particularly interesting because it shows the convergence of thinking on the problems of behaviour in both physiology and psychology at that time.

It is nearly 25 years since Humphrey's book on learning appeared, and it is over 25 years since the first of his remarkable papers on homeostasis (or "systems") was written, yet it is only relatively recently that the concept of homeostasis has swept into the front pages, as it were, of psychology. In the introductory textbook by Stagner and Karwoski which uses the principle of homeostasis as basic for the presentation of the phenomena of behaviour, Humphrey is referred to only once and then in an entirely different connotation. Yet Humphrey is really the originator of this approach in psychology, a fact for which he has not been given sufficient credit, and one which should rank as a major Canadian contribution to current thinking in psychology.

I propose now to summarize Humphrey's views as they are found in his book on learning (12), and then to mention some of the inherent difficulties. To my mind the most significant thing about Humphrey's work is that it is the first and by far the most important attempt to provide a link between the apparently widely divergent basic frameworks that existed at the time he wrote. It is often forgotten today what poles apart the various theories (what might be called "models" today, I suppose) seemed to be to the student of psychology in the 1920's, how utterly incompatible some views seemed with others. Humphrey was to provide a bridge between them.

The first problem with which he dealt was that of "direction"—whether in fact organic events have a "direction" not possessed by inorganic events. In the case of organic events "the striking fact is not the dependence of events of today upon those of yesterday, but the direction of that dependence, which is towards the conservation of the biological entity in question" (12, p. 3). Humphrey's method of dealing with this problem was first to show that the property of conservation exists not only in organic bodies but also in inorganic systems, such as the pendulum, and in chemical systems such as buffer solutions, and then to suggest that the "*organism may be said to behave as an intricate system of material processes, tending actively to maintain a complex pattern under constantly changing conditions*" (12, p. 41). The emphasis in this description lies on the words "system" and "pattern." If organisms are systems they are like pendulums or the tides; where they differ is in being composed of different material particles at different times. The sameness, that is, is one of pattern rather than of material particles. In this respect they may be compared with such a chemical system as phosphorus pentachloride. When you heat a solution of phosphorus

pentachloride to 250°, 80 per cent of the solution splits up into the trichloride and chlorine, leaving only 20 per cent of the pentachloride. But the whole system is in a state of flux, an individual atom of chlorine being free at one moment, while at the next it may form part of a pentachloride molecule. It is only the pattern and the proportions that remain the same. The living system goes even further: a closer analogy to it is the candle flame which in still air retains the same form from minute to minute while being composed of different material particles all the time.

Having prepared the way by thus describing his conception of the living organism as a system, Humphrey turns to the general problem of learning and puts forward the view that here again no new principles need be involved. If the process of learning is regarded as a whole, it is seen to be an adjustment of the organism to its environment which becomes only relatively complete when the action is learnt. The temporal factor is as indispensable an element in learning as the spatial factor is in perceiving. "A mouse has 'learnt' to run a simple maze by traversing it, let us say, fifteen times. From the point of view of physical science the successive stimulus-complexes formed by the thrice-daily presentations of the maze are discrete in time. As stimuli-in-relation-to-the-mouse they must be considered in connection with each other; and unless there occurs in the animal some kind of synthesis to which they are related, learning cannot take place" (12, p. 113–4). Negative adaptation, for instance, is one of the clearest examples of the necessity for treating the organism as a four-dimensional continuum; and the same concept is applied to the conditioned reflex and to maze learning.

In general, learning may be said to be a process by which the organism attains or returns to a state of equilibrium in a new environment, and the organism as a system possesses a particular kind of organization through the action of which this becomes possible. As long as the organism is out of equilibrium with, and badly adjusted to, its environment, forces persist which tend to bring it again into a state of equilibrium, and to reduce the output of energy in that situation—such as locomotion in a maze—to a minimum. As soon as the system has attained or returned to equilibrium, learning can be regarded as having taken place.

The originality and novelty of Humphrey's book (in relation to the time at which it was written) lie especially in four directions: (1) the idea of the organism as a system with a particular kind of organization; (2) the four-dimensional notion in the theory of learning; (3) the application of the same principle to what many psychologists at the time regarded as qualitatively different types of learning; and (4) the notion

of learning as the attainment of a homeostatic equilibrium between a system and its environment.

Of course, Humphrey's proposals were not without critics, the most formidable of whom was undoubtedly Koffka (14). Although Koffka was sympathetic to Humphrey's view of the organism as a system, he pointed out several weaknesses in Humphrey's application of this principle to the nature of learning. For one thing, the mere recourse to four-dimensional space-time provides no explanation of why certain things are bound together in the process of learning and other things are not. If you learn to type, the separate series of trials may be thought to form a continuous and increasing approximation to a state of equilibrium, but this does not explain what it is about the different trials that binds them together and separates them from all the other things that the individual does apart from learning to type. Furthermore, not all objects "learn" although they behave in a four-dimensional continuum. A billiard ball returns to its previous shape after each deformation from the billiard cue, and remains "true" even after many years of use. For if the past does not exist. What is it that distinguishes the things that "learn" from the things that do not learn?

There are other difficulties, too, in Humphrey's theory. How can one identify the state of equilibrium attained by an organism that has learnt something, except *ex post facto*? How can one know whether the changes undergone during learning increase the equilibrium or not? What will our criterion be? When can we say the process of learning has been completed? And so on.

Yet this, of course, is as it should be. Any new theory or new approach is bound to run into difficulties at first. The advantage is that it starts people's minds off in a new direction, helps new problems to be formulated and new experiments to be designed.

After he had completed his book on learning Humphrey went on sabbatical leave to Cambridge during the session 1933–4. There F. C. Bartlett suggested to him that he write a book on thinking. Humphrey's first answer to this was to write the chapter on "Thought" in Boring, Langfeld, and Weld's 1935 textbook. For the next four or five years he settled down to his task of writing the book. Unfortunately, for a number of reasons the publication of the book was held up until 1951, though a shorter and much more popularly written book, *Directed Thinking*, was published in 1948. When his scholarly textbook (13) appeared it was widely reviewed and praised in the most important scientific journals. The *British Journal of Psychology* devoted a notice of some five thousand words to it; the *Psychological Bulletin* and the *American Journal of Psychology* gave it long and thoughtful notices.

The book was remarkable in many ways, but one of its most important contributions was that it provided the first full-scale discussion in English of the work of the Wurzburg group, work which most students had previously had to learn about through Titchener's abbreviated and hostile description (16). Whatever one may think today of the value of the Wurzburg approach, the importance of the problems they were concerned with, or the technical aspects of the methodology they employed, there is no doubt of the school's historical importance in breaking the stranglehold that Wundt had established on experimental psychology in Germany. Description of the Wurzburg work makes up about half the contents of Humphrey's book and is followed by a discussion of the experiments of the Gestalt school and of other workers of more recent generations. The arguments and descriptions are brought together in the last chapter, which surveys fifty years of experiment on the psychology of thinking and summarizes the conclusions in sixteen statements. Here again one may see the characteristic contribution that Humphrey has made to psychology throughout his career, in the way in which the different approaches to the problem are related and their points of common agreement brought out.

Now George Humphrey has retired from his official position. In spite of numerous offers from several countries he has decided, temporarily I hope, not to accept any further official appointment. I hope that Canadian psychologists will not allow the credit for appreciating the work of this remarkable man to be claimed wholly by Britain, the United States, and Germany, and that they will not soon forget the contributions of one of the small band of men who have put them in the position they enjoy today.

REFERENCES

1. Cannon, W. B. Organization for physiological homeostasis. *Physiol. Rev.*, 1929, **9**, 399–431.
2. Humphrey, G. The conditioned reflex and the Freudian wish. *J. abnorm. soc. Psychol.*, 1920, **14**, 388–392.
3. Humphrey, G. Education and Freudianism. *J. abnorm. soc. Psychol.*, 1921, **15**, 350–386.
4. Humphrey, G. The conditioned reflex and the elementary social reaction. *J. abnorm. soc. Psychol.*, 1922, **17**, 113–119.
5. Humphrey, G. The theory of Einstein and the Gestalt-Psychologie: A parallel. *Amer. J. Psychol.*, 1924, **35**, 353–359.
6. Humphrey, G. The psychology of Gestalt. *J. educ. Psychol.*, 1924, **15**, 401–412.
7. Humphrey, G. Is the conditioned reflex the unit of habit? *J. abnorm. soc. Psychol.*, 1925, **20**, 10–16.
8. Humphrey, G. The effect of sequences of indifferent stimuli on a reaction of the conditioned response type. *J. abnorm. soc. Psychol.*, 1927, **22**, 194–212.

9. HUMPHREY, G. Le Chatelier's rule, and the problem of habituation and dehabituation in *helix albolabris*. *Psychol. Forsch.*, 1930, **13**, 113–127.
10. HUMPHREY, G. Extinction and negative adaptation. *Psychol. Rev.*, 1930, **37**, 361–363.
11. HUMPHREY, G. Learning and the living system. *Psychol. Rev.*, 1930, **37**, 497–510.
12. HUMPHREY, G. *The Nature of learning*. London: Kegan Paul, 1933.
13. HUMPHREY, G. *Thinking: An introduction to its experimental psychology*. London: Methuen, 1951.
14. KOFFKA, K. *Principles of Gestalt psychology*. London: Kegan Paul, 1935.
15. SCHLOSBERG, H. A study of the conditional patellar reflex. *J. exper. Psychol.*, 1928, **11**, 468–494.
16. TITCHENER, E. B. *Lectures on the experimental psychology of the thought-processes*. New York: Macmillan, 1909.
17. UPTON, M. The auditory sensitivity of guinea pigs. *Amer. J. Psychol.*, 1929, **41**, 412–421.
18. WEVER, E. G. The upper limit of hearing in the cat. *J. comp. Psychol.*, 1930, **10**, 221–234.

EFFECTS OF THE PRESENCE AND DISCUSSION OF OTHERS ON EXPRESSED ATTITUDES[1]

W. E. LAMBERT AND F. H. LOWY
McGill University

IT IS an old and well-established fact in social psychology that individuals' judgments, when measured in group situations permitting communication, differ from their judgments when measured in alone situations ("alone" commonly meaning an individual in the presence of an experimenter). The difference characteristically takes the form of a "regression" of judgments towards a group mean. A multitude of interpretations are given to explain this finding. The experiments examined by the present authors (4, 5, 12, 15, 16, 17) all demonstrate judgmental regression, but there is no conclusive evidence in any of them that actual perceptual changes also occur; the possible exception in some of Asch's subjects (4) has been seriously questioned by F. Allport (3, pp. 367f.). When one examines the types of judgments required, one realizes the hazards of generalizing the principle of judgmental regression to social behaviour. In the experiments mentioned above, subjects were asked to judge the lengths of lines, the sizes of rectangles, the number of beans in a bottle, the number of clicks heard, the imagined movement of a spot of light, and the lengths of sticks, respectively.

It is not only the "communicating" feature of the group that leads to judgmental regression, for F. Allport (2) reports that the mere proximity of co-workers, with no communication permitted, tended to moderate judgments of the pleasantness or unpleasantness of odours and of the heaviness of weights.

The purpose of the present study is to determine if *attitudinal* responses, the nature of which constitutes a central problem in social psychology, are susceptible to regression towards the group mean as judgments of external stimuli have been shown to be. Farnsworth (8) asked college students to judge eighteen statements as to their degree of pacifism or militarism, testing different samples of students in the alone and in the group situations, and concluded that there was no marked difference in their judgments. LaPiere and Farnsworth later stated that "no . . . tendency toward social conformity appeared in [this] study of

[1]The authors wish to thank Professors G. A. Ferguson and J. D. Ketchum for their generous assistance in the organization and presentation of this study, and Mr. R. Gardner who kindly checked the statistical findings.

attitudinal judgments" (14, p. 540). In this experiment, subjects were not asked to give their own attitudes but merely to categorize attitude items; even so, the fact that twelve of the eighteen items show reduced variance in the group situation is, to the present authors, at least suggestive of a conforming tendency. A study by Burtt and Falkenberg (7) indicates that "religious attitudes" can be changed when subjects are informed of the attitudes of the majority of church members or of a group of ministers. More recently, Helson *et al.* (10) have shown that attitudinal responses to peace and war, using the same scale as Farnsworth (8), do regress towards a group average when subjects are tested in a simulated group situation permitting communication.

To facilitate comparisons with methodologically related studies, the amount of communication permitted in the present experiment was varied systematically. Since several recent studies and reviews have pointed out the importance of the social structure of the group in determining degree of influence among members—especially the effect of acquaintance on mutual influence (5, 6, 8, 13)—this factor was included in the design.

Procedure

The F scale (1), a recommended test of general prejudice (11, p. 75), was chosen as the measure of attitudes because it was constructed to tap deep and persistent individual attitudes; it measures prejudice without appearing to do so (that is, with few references to particular minority groups); it has high reliability (.90); and it permits a sizeable range of responses. Thirty items, taken from forms 40 and 45 of the original work, were reworded to include references to Canada and divided into three sub-scales, roughly equated with respect to the discriminatory power of the 10 items placed in each. The order of presentation of the 3 sub-scales was randomized for all Ss.

The Ss were 102 undergraduates of McGill University. Experimental treatment was given to 65 male Ss, distributed into 13 five-man groups, while the remaining 37 individuals served as controls. Some of the experimental Ss were selected because they were members of recognized groups (habitual teams of card players, fraternity brothers, etc.), others because they were unlikely to be acquainted with one another. In the five "High Acquaintance" groups Ss had known each other for at least one year and came together socially either outside the university or at it, or both. In the eight "Low Acquaintance" groups Ss did not come together socially and were acquainted, if at all, only through common university experiences.

Experimental Ss answered one of the sub-scales in each of three situations.

(*a*) *"Alone" situation.* They were first approached individually in an informal manner and asked to fill out the questionnaire for the purpose of "test standardization," without signing their names. After having completed the form, each S was told that *E* was standardizing a number of these questionnaires and would appreciate it if S would fill out another similar one in a few days' time.

(*b*) *"Together" situation.* When given the second questionnaire, Ss were seemingly unaware that a group experiment was taking place. All five were seated so that no

one could readily see what his neighbour was writing. They were informed that another questionnaire, being standardized for use with college students, would be given them, and instructed to answer truthfully, not to give their names, and not to talk at all.

(*c*) *"Discussion" situation.* When all papers had been collected, Ss were asked to fill out one more form while they were together, but to discuss each question separately, so that *E* could determine which questions "made sense." Before any answer was written *E* read each question and asked for opinions. Discussion, in which each S was prompted to express his attitudes, was limited to approximately five minutes per question, after which Ss were asked to write their answers.

The 37 control Ss never met each other in the course of the experiment. Each S filled out the questionnaire, with only an experimenter present, on three separate occasions. With this control, it was possible to determine whether any changes which might occur in the experimental group were due to the experimental conditions, or were simply a function of time and practice.

Results and Discussion

The term "regression" refers to a moving toward the average, and the standard deviation of scores becomes the measure of such movement (18). In the light of previous research on the regression of judgments, we predicted that deviant attitudinal responses would be similarly modified by discussion with others, and possibly by their mere presence.

TABLE I

Variability in F-Scores* in Relation to Conditions of Group Administration

Group		Condition: Alone	Togeth.	Disc'n	*P* values†: σ_A vs. σ_T	σ_A vs. σ_D
All experimental	SD	10.43	10.43	8.42	n.s.	.01
Ss (*N* 65)	M	33.20	33.03	33.60		
	r		r_{A-D} .81			
High Acquaint-	SD	10.56	8.08	7.06	.01	.001
ance (*N* 25)	M	32.56	29.80	32.16		
	r	r_{A-T} .90	r_{A-D} .88			
Low Acquaint-	SD	10.31	11.19	9.06	m.s.	n.s.
ance (*N* 40)	M	33.60	35.05	34.50		
	r	r_{A-T} .89	r_{A-D} .80			
		Test I	Test II	Test III	σ_I vs. σ_{II}	σ_I vs. σ_{III}
Control *Ss*	SD	9.34	9.65	10.18	n.s.	n.s.
(*N* 37)	M	31.19	30.59	29.65		

*Scores could range from 10 (least prejudiced) to 70 (most prejudiced).

†From two-tailed *t*-tests, taking into account the correlation between conditions (18, p. 190).

Table I shows the means and sigmas of attitude scores for all subjects under the various situational conditions. Considering all experimental Ss together, there is a significant change in the variability of attitude scores from the alone to the discussion situation, a finding in agreement with the consensus of previous related research. There is, however, no reduction of variability as a consequence of merely being together, without communication with others.

Among High Acquaintance Ss there is a significant reduction of score variability from both the alone to together and alone to discussion situations, while Low Acquaintance Ss show no significant reduction in either.

Looking more closely at the High Acquaintance Ss, we see that their *mean attitude score* is significantly smaller (indicating less prejudice) in the together situation than in either the alone situation ($t = 2.79$, $p = .01$) or the discussion situation ($t = 3.02$, $p = .01$). The sequence of findings indicates that the High Acquaintance Ss became more homogeneous in attitudinal responses and less "prejudiced" in the together situation, but increased their mean level of prejudice in the discussion situation, while maintaining their attitudinal homogeneity ($\sigma_T > \sigma_D$, not significant).

The above findings are to be compared with those for control subjects, which show no reliable change in either sigmas or mean scores from one testing situation to another, indicating that no systematic changes in scores are likely as a function of repeated testing, *per se*.

The over-all results confirm the hypothesis that a discussion situation prompts attitudinal regression towards the group mean, but they do not support the hypothesis that the mere proximity of others has the same effect. For Ss who know each other well, however, the proximity of others produces attitudinal regression, whereas, when the "others" are not well known, attitudinal responses are not likely to be modified even when discussion is permitted.

The data indirectly support the hedonistic notion that individuals will modify their attitudinal responses if some personal advantage is contingent upon a change. In the present experiment, High Acquaintance Ss presumably associated with one another habitually because they benefited in various ways from interaction. In the light of past experiences with one another, these Ss could estimate an acceptable range of attitudes for their group by merely noting who was sitting with them. They may have adjusted their responses to this acceptable range anticipating that the questionnaire might be discussed in the group on some future occasion, when a reported deviance from the accepted range could meet with disapproval and thus threaten their status in the group. Furthermore, it would appear that these Ss made a conservative estimate of the

mid-point of the acceptability range in the together situation, and then re-evaluated the position of this point when discussion was permitted. This last interpretation is similar to that offered by Bovard (5, pp. 403f.) to explain analogous findings.

The fact that the Low Acquaintance Ss, who did not habitually interact and were unlikely to meet again, were not attitudinally perturbed by either the presence or discussion of others gives further support to our hedonistic interpretation of social influence. They behaved as though they had nothing to gain from modifying their expressed attitudes.

Summary

Sixty-five male undergraduates filled out comparable forms of the F scale (1) under three conditions: alone, in groups of five without discussion, and in groups of five after discussion. The over-all findings indicate that group discussion produces a significant reduction in variability of attitude scores, but the mere presence of others does not. When attention is given to the degree of acquaintance among *Ss*, however, a significant reduction in score variability is evident for High Acquaintance Ss from the alone to the together situation, where no communication is permitted. Low Acquaintance Ss appear attitudinally unaffected by either the presence or discussion of others. A hedonistic explanation is offered for the findings.

REFERENCES

1. Adorno, T. W., Frenkel-Brunswik, E., Levinson, D. J., & Sanford, R. N. *The authoritarian personality.* New York: Harper, 1950.
2. Allport, F. H. *Social psychology.* Boston: Houghton Mifflin, 1924.
3. Allport, F. H. *Theories of perception and the concept of structure.* New York: Wiley, 1955.
4. Asch, S. E. Effects of group pressure upon the modification and distortion of judgments. In Swanson, G. E., Newcomb, T. M., & Hartley, E. L. (eds.), *Readings in social psychology.* New York: Holt, 1952.
5. Bovard, E. W., Jr. Group structure and perception. *J. abnorm. soc. Psychol.*, 1951, **46**, 398–405.
6. Bovard, E. W., & Guetzkow, H. A validity study of rating scales as a device to distinguish participants in stable and temporary groups. University of Michigan: Conference Res. Reports, August, 1950, (mimeo.).
7. Burtt, H. E., & Falkenberg, D. R., Jr. The influence of majority and expert opinion on religious attitudes. *J. soc. Psychol.*, 1941, **14**, 269–278.
8. Farnsworth, P. R. Further data on the obtaining of Thurstone scale values. *J. Psychol.*, 1945, **19**, 69–73.
9. Festinger, L. Informal social communication. *Psychol. Rev.*, 1950, **57**, 271–282.
10. Helson, H., Blake, R. R., Mouton, J. S., & Olmstead, J. A. Attitudes as adjustments to stimulus, background, and residual factors. *J. abnorm. soc. Psychol.*, 1956, **52**, 314–322.

11. Hyman, H. H., & Sheatsley, P. B. *The authoritarian personality:* A methodological critique. In Christie, R., & Jahoda, M. (eds.), *Studies in the scope and method of "The authoritarian personality."* Bloomington, Ill.: The Free Press, 1954.
12. Jenness, A. The role of discussion in changing opinion regarding a matter of fact. *J. abnorm. soc. Psychol.*, 1932, **27**, 279–296.
13. Kelley, H. H., & Thibaut, J. W. Experimental studies of group problem solution and process. In Lindzey, G. (ed.), *Handbook of social psychology*, vol. 2. Cambridge, Mass.: Addison-Wesley, 1954.
14. LaPiere, R. T., & Farnsworth, P. R. *Social psychology.* New York: McGraw-Hill, 1949.
15. Olmstead, J. A., & Blake, R. R. The use of simulated groups to produce modifications in judgment. *J. Pers.*, 1955, **23**, 335–345.
16. Sherif, M. Group influences upon the formation of norms and attitudes. In Swanson, G. E., Newcomb, T. M., & Hartley, E. L. (eds.), *Readings in social psychology*. New York: Holt, 1952.
17. Vaughan, W. F. An experimental class demonstration of suggestibility. *J. abnorm. soc. Psychol.*, 1935, **30**, 92–94.
18. Walker, H. M., & Leo, J. *Statistical inference.* New York: Holt, 1953.

MASS MEDIA, LEARNING, AND RETENTION[1]

D. C. WILLIAMS, J. PAUL,[2] AND J. C. OGILVIE[3]
University of Toronto

MASS MEDIA may be defined as means of communication in which various transmitting mechanisms permit a speaker or writer to communicate with a large segment of the population without the necessity of being physically in the presence of his audience. Thus, the mechanization of writing produced the printed page, the prestige of which grew to the point where books and literacy have become synonymous with "culture." The past fifty years, however, have witnessed a rapid increase in the number and variety of such mass media. The mechanization of listening by the radio, and of vision by the film and, more recently, by television, have challenged in quick succession the former monopoly of the printed word.

The growth of these media has produced an increasing interest in the study of their effects. Most of the study so far has been in the form of applied research, the investigators concerning themselves primarily with the effectiveness of the various media in selling soap, attitudes, or the Republican party. Less study has been given to the media as educational rather than commercial devices. Although laboratory presentations to single-sense modalities have been studied (1), using both simple and complex materials (2), no attempts have been made to compare retention of complex material presented both visually and aurally (as in television) with retention resulting from presentation to a single-sense modality.

The study to be reported grew out of an interdisciplinary seminar on Communications and Culture, composed of one staff member from each of the departments of anthropology, economics, English, psychology, and town planning, together with from two to four graduate students from each department.

[1]This study is one product of a two-year interdisciplinary seminar on Communications and Culture, held at the University of Toronto, 1953–5, under the sponsorship of the Ford Foundation. The present authors acted as agents of the seminar in designing the experiment. Acknowledgments are due to Professors E. S. Carpenter, W. T. Easterbrook, H. M. McLuhan, and their students, without whose participation the problem would never have arisen, and to the Canadian Broadcasting Corporation for the generous provision of facilities. Aspects of the study have already been reported for general readers in *Explorations* (3, 4).

[2]Now at the University of Western Ontario.

[3]Now at Defence Research Medical Laboratories, Toronto.

In the course of the weekly meetings, the question arose as to whether the inherent characteristics of different mass media would produce differential effects on learning. The opportunity to answer the question experimentally arose when the Canadian Broadcasting Corporation generously agreed to put its facilities at our disposal while one of our number (Professor E. S. Carpenter) was recording by kinescope an address for subsequent telecast. This address, of approximately 23 minutes, was entitled "Thinking through Language," and consisted of an analysis of the manner in which the apprehension of external reality is conditioned and determined by the formal characteristics of the language one speaks.

The specific purpose of the experiment was to compare the efficiency of television, radio, and the printed page in imparting information. The study was designed to provide a comparison of the effects of the media in terms both of immediate learning and of retention over several months. The material used, a lecture of approximately 3,200 words, made the experiment also relevant to the controversial issue of the relative efficiency of presentation through various sense modalities of complex and lengthy material.

Method

The basic design consisted of the simultaneous presentation of the same lecture through the different media to various audience groups equated for certain relevant variables. Immediately after the lecture, each group wrote an objective multiple-choice examination on its content. The experiment was carried out in the Toronto studios of the CBC. The technical facilities available permitted comparison of the three media groups (TV, radio, and print), and a fourth group, consisting of a "live audience," present in the studio.

Subjects

The subjects were 108 second-year undergraduates in the General Course in Arts at the University of Toronto, all of whom were studying anthropology as one of five courses comprising their year's work. The lecture topic was unfamiliar to them. The class list was arranged in descending order of academic grades (based on first-year results) and randomly divided into 4 groups of 27 on a stratified sampling basis, so that each group contained an equal number of high, average, and low students. "High" meant grades of A and B+, "average" meant grades of B and B—, "low" meant grades from C+ down to the minimum passing grade.

Procedure

Each of the four groups was randomly assigned to one medium; television, radio, reading, or listening to the lecturer. The assignments were announced to the students on their arrival at the studio, and each group went to a separate room. To secure optimum motivation the class instructor had agreed to incorporate the students' performance on the subsequent examination into their course term marks. However, to avoid undue anxiety, it was arranged that those who did well would get a term mark bonus, while those who did poorly would suffer no penalty. This arrangement presumably helped to produce the 100 per cent attendance obtained, and to offset, if not eliminate, the effects of personal preference for one or another medium.

The lecture was delivered in the physical presence of the studio audience and was simultaneously relayed to the radio group, who heard it over a loudspeaker, and to the television group, who viewed a conventional set. At the same time mimeographed copies of the lecture were distributed to the reading group, who read it at their own pace, but only for the length of time it took to deliver the lecture. No note-taking was allowed in any group.

Immediately after the conclusion of the lecture each group wrote a 30-minute examination. This consisted of 19 multiple-choice questions (4 alternatives each) and of one broad essay-type question, to be answered in 200 to 300 words. Most students finished well before the time limit.

In order to test the differential effect of various media on retention over a prolonged period of time, the same multiple-choice questionnaire was administered without warning to the same class eight months later. During the interval some students had heard the lecture a second time when its kinescope was shown on a television programme and some may have discussed it with friends. It was assumed, however, that such reinforcement would take place randomly across the four audiences.

In order to minimize the advantages inherent in certain media which cannot be duplicated in others, no pictures, slides or other visual aids were used in the television presentation. The lecture dealt with abstract concepts which did not appear to favour presentation by any particular medium, and it was memorized so that the reading group would receive exactly the same content as the others. In order to compensate, within the limits of print, for the fact that the reading audience was deprived of both the inflections and gestures of the lecturer, certain key words in the mimeographed material were capitalized to give something of the same emphasis they received as delivered (for example, "I FOLLOW THE THREAD of your argument").

RESULTS

Immediate Retention

The results here given are confined to an analysis of the multiple-choice section of the examination.[4] The test was administered immediately after the lecture. An analysis variance of the test results (Table I) revealed significant differences between the four audience groups ($p < .01$).

TABLE I

ANALYSIS OF VARIANCE OF CORRECT ANSWERS OBTAINED IMMEDIATELY AFTER LECTURE (19 MULTIPLE-CHOICE QUESTIONS)

Source of variation	*df*	*SS*	*MS*	*F*	*p*
Media	3	96.5	32.17	6.80	.01
Grades	2	56.9	28.45	6.01	.01
Interaction	6	48.8	8.13	1.72	n.s.
Residual	96	454.0	4.73		
TOTALS	107	656.2			

[4]Students' marks on a preliminary grading of the essay question showed a correlation of +.89 with their marks on the multiple-choice questions.

Academic achievement, used as a control variable, was also found to be significant ($p < .01$).

Application of the *t*-test to the differences between individual media groups showed that the mean score of the TV audience was significantly better than that of the radio group. The score of the radio group was in turn significantly better than that of the reading group. No significant differences were found between the reading and studio groups (Table II).

TABLE II

MEAN SCORES OF THE VARIOUS MEDIA GROUPS (IMMEDIATE RETENTION) AND p VALUES FOR DIFFERENCES BETWEEN THEM

Group	N	Mean	p
TV	27	14.7	
			.01
Radio	27	13.2	
			.05
Reading	27	12.4	
			n.s.
Studio	27	12.3	

Inspection of the differential effects of various media on students of different academic grades suggests that TV may have had the most pronounced effects on the best students (see Figure 1). However, this can only be regarded as a suggestion for future research, since the interactions in Table I were not significant.

Retention after Eight Months

The original test was readministered eight months later to the 74 students available of the 108 who took part in the first experiment. The difference in numbers was due to the fact that some of the students did not re-register in this course, and some were absent at the time the recall test was given. To determine whether the 74 students re-examined were representative of the original 108, the performance means of the two groups on the first test were compared (Table III). Although the means of the re-test groups were slightly lower for each medium, the differences were fairly uniform and not great.

The eight months which elapsed between the lecture and the second test included a summer vacation period, which gave the students ample opportunity to forget about their studies in general and this lecture in particular. No student had any idea that a repetition of the examination was intended.

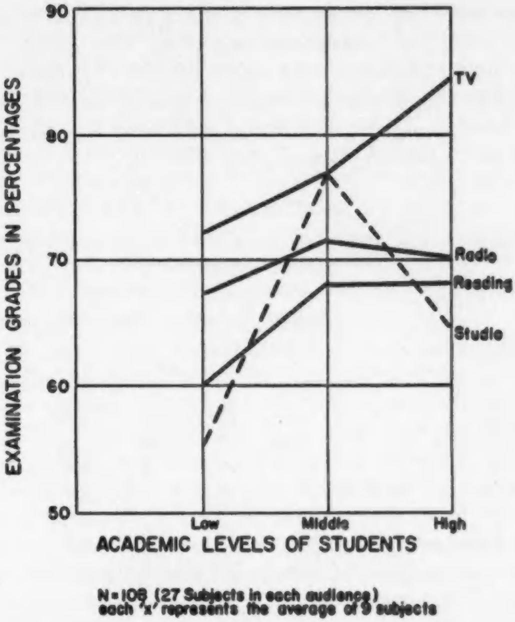

FIGURE 1. Relation between academic levels of subjects and scores obtained (on original test) on each medium.

Since the subjects of this experiment were arts students, it was assumed that the number of correct answers given by such a sample would be better than chance expectation, even if they had not seen or heard the lecture. To estimate how far the results of the recall experiment were due to general knowledge rather than to information obtained from the lecture, a control group was used. The questionnaire was given to 15 Second Year Honour Psychology students, whose general training in social science was similar to that of the experimental group, but who had not heard the lecture or received previous instruction from the lecturer.

Two questions were considered: (1) were the media differences demonstrated by the first test still demonstrable after eight months? (2) did the media have a differential effect on forgetting during this period?

Media differences. An analysis of variance of the results of the second test showed that there were still significant differences between the means of the various media groups after a period of eight months ($p < .01$).

Since there were unequal losses of subjects in the four groups, further comparisons were not statistically justifiable. The answer to the first question is, however, clear; after eight months, significant differences existed between the groups exposed to the different media. The results showed one change in the rank order of the four media: the studio group moved from last to second place (Table III).

TABLE III

COMPARISON OF MEAN SCORES OF RE-TEST GROUPS ORIGINAL GROUPS AND CONTROL GROUP

	Re-test Groups			Original Groups	
Group	Test 1	Test 2	N	Test 1	N
TV	14.3	11.7	14	14.7	27
Radio	12.4	10.0	21	13.2	27
Reading	12.1	9.0	18	12.4	27
Studio	12.0	10.6	21	12.3	27
Control		6.8	15		

Differential forgetting. In order to answer the second question, it is necessary to compare the differences between the first and second tests for each group. The respective losses (Table III), when tested by analysis of variance, could not be considered to differ significantly from each other ($p > .05$). This implies that the amount retained after eight months was proportional to the amount originally learned. In other words, the rate of forgetting information was apparently independent of the medium by means of which it was acquired. This was graphically demonstrated by the fact that the original ranking of media using mechanical transmission—television, radio, print—still held after eight months.

Since it was found that for every group the mean score for the second test was significantly lower than for the first test, a third question was asked. If, after eight months, the subjects have in general lower scores, how much better than intelligent guessing are their second test results? This is answered (Table III) by comparing their results with those of the control group of psychology students. The scores of the control group are better than random guessing, but significantly lower than the lowest of the four media groups ($p < .05$).

DISCUSSION

The results support the hypothesis that, under the conditions described, media do influence retention in terms both of immediate memory and of memory over a period of several months. The superior results of the

television audience support the findings of previous experiments (1, 2), carried out before the advent of television, that presentation of material by means of two sense modalities is more effective than either simple visual or aural presentation. Further, the results suggest that this superiority exists even with such complex and lengthy material as was used in the present experiment.

The use of an additional group to whom the same material was presented in the form of a traditional classroom lecture would have been of great interest. It was originally hoped that the studio situation would provide such a comparison, but one glance at the confusion and distractions of the television studio made it clear that it was in no way similar to a classroom. Hence it was decided that no valid classroom generalizations could be drawn from the results of this group.

The results of this experiment, as usual, raise more questions than they answer. The novelty of television as compared with other media may have been a relevant variable. The results might be different with noncaptive audiences, or with subjects of different educational background. Results might be different if media advantages were maximized, rather than minimized as they were in this experiment. Finally, further research is needed to determine the particular types of subject-matter whose retention is facilitated by any particular medium of communication.

Summary

An experiment was performed to compare the effects of different mass media on learning. The subjects, 108 undergraduates, were divided into four groups, equated for academic standing. A lecture was communicated simultaneously to all four groups, in different rooms, through television, radio, reading, and listening to the lecturer in the studio. An objective examination was given to all four groups immediately after the lecture, and again eight months later. It was found that the medium used made a significant difference in retention in both examinations. TV, radio, and reading ranked in that order of effectiveness, and the order was unchanged in the second test, eight months later.

REFERENCES

1. Hovland, C. I., Lumsdaine, A. A., & Sheffield, F. D. *Experiments on mass communication.* Princeton Univer. Press, 1949.
2. Klapper, J. T. *The effects of mass media.* New York: Columbia Univer. Bureau of Applied Social Research, 1949 (mimeo.).
3. Paul, J., & Ogilvie, J. Mass media and retention. *Explorations,* 1955, no. IV, 120–123.
4. Williams, D. C. Experiment in communication. *Explorations,* 1954, no. III, 75–82.

USE OF PSYCHOLOGICAL CONSTRUCTS FOR IMPROVING SELECTION TEST VALIDITY[1]

W. N. McBAIN
McGill University

To WHAT EXTENT are the construction and use of personnel selection tests a psychological occupation? In their review of the Industrial Psychology of 1954 (10) Wallace and Weitz admit their failure to detect ". . . thinking . . . about the broad theoretical basis of psychology's efforts to improve industry's utilization of human resources."[2] A study of newly published tests reveals marked advances in such details as printing and format, ease of administration and scoring, statistical sophistication of interpretive techniques, and lucid presentation of testees' results. But the inspiration and justification of such tests are much more likely to relate to filling an industrial need than to the evolution of a theoretical viewpoint. It is through association with academic discipline and theory that industrial psychologists justify their professional status; there is considerable question whether their work reflects this association.

The purpose of this paper is to discuss the problem of improving the validity of selection tests. There has been too little recourse to theoretical psychology by business and industrial psychologists who construct and administer personnel selection tests. If such persons wish to consider themselves "applied psychologists," they have a responsibility for applying *psychology*, for translating its concepts into operational terms, and for building these concepts into the tests they construct and interpret. Neither statistical sophistication nor the ability to determine what the business man wants is sufficient qualification for such a title. Even more important is the likelihood that re-emphasizing psychological principles may be worthwhile in a pragmatic sense because it will help increase the accuracy of test prediction. Before considering this, however, one should inquire into the factors which inhibit the predictive power of present selection techniques.

[1]Paper read at the Annual Meeting of the Canadian Psychological Association, Toronto, Ontario, June 6–8, 1957.

[2]See also Heron's (8) article, in which he considers this and related problems in constructive detail. His 18 references represent a thoughtful selection of views on this question from both British and American sources.

Factors Lowering Test Predictive Power

Unreliability has been the source of prediction inaccuracy most investigated. It is clear that if a test cannot predict accurately performances of the type it samples one cannot place much reliance on its prediction of other variables. More recently stress has been placed upon a like need for reliability in the criterion measure, that is, in the measure of accomplishment which test scores are intended to predict. Bingham (2) has gone so far as to suggest that published test validities should recognize the unreliability of the specific criterion predicted by correcting for the attenuation attributable to that source, in much the same way that corrections are published for known restriction of range in performance. This should provide a more uniform basis on which to judge the relative validities of tests.

Less attention has been paid to the questions of relevance and representativeness in the criterion.[3] This may be because, as Thorndike states (9, p. 123), "the ultimate criterion of success in any duty must always be determined on rational grounds." Any immediate or intermediate criterion has relevance (and hence authority) only as it relates to the ultimate criterion, and the latter is a matter of policy and decision rather than of measurement and statistical manipulation. It may be such a vague idea as "the good of the company," surely an intimidating concept to the psychometrician. Nevertheless, since success or failure of employees will ultimately be judged on this basis, immediate or intermediate criterion measures peripheral to it will lack relevance, no matter how convenient, quantifiable, and desirable in other ways they may be. Nor can they be considered adequate unless they are representative, in that they contain to some extent a balance of all the important factors in the ultimate criterion. Such criteria are matters of policy, hence likely to be unique to a particular situation. But, since an examination of validity coefficients will not necessarily disclose lack of either relevance or representativeness, one can only point to the ultimate criterion as the logical starting place in determinations of functional validity.

[3]As customarily used, "ultimate criterion" is conceptually some determination of the eventual suitability of the testee. "Immediate criterion" refers to a measure of success relatively proximal to the test itself, as "rate of production at the end of six months." Intermediate criteria are measures secured over a relatively longer term. The immediate, intermediate and ultimate criteria for the selection of entrants to a medical school might be, respectively, grades at the end of the first year, graduation or non-graduation from medical school, and "success in medical practice." The nebulous and non-specific character of most ultimate criteria makes them difficult to quantify; hence most validity studies are based on immediate or intermediate criteria.

When the reliability of both tests and criteria has been established, and adequate consideration given to relevance and representativeness, one may still obtain low or insignificant validity coefficients. An approach often used in such cases has been to give up further attempts to increase validities and attempt to compensate by a decrease in the selection ratio, that is, the number of persons selected relative to the number applying—a sort of *crême de la crême* procedure.

As suggested by Haire (7, p. 153), there is a limit to this method of increasing test efficiency, for while improving the quality of selected individuals it increases wastage. In a period of high employment (i.e., when there is a "tight" labour market) there simply are not sufficient applicants available from whom to select the required number of superior workers. And since it is particularly important under such conditions to select only the best of those available, the low selection ratio is not a substitute for the relatively greater precision of more valid tests. Even with a greater pool of applicants from which to select, some consideration should be given to the non-immediate economic aspects of this type of wastage. Potentially adequate workers summarily rejected because of test error are not usually considered to be of further concern to the personnel department. But such individuals are likely to feel resentment towards the organization, and this may be freely expressed, with consequent discouragement of other promising applicants. The result may be a further reduction of the labour supply. What is more, applicants who consider themselves unjustly rejected are unlikely to return for a second try, even though conditions may have changed so that they are now needed. The use of an extreme selection ratio, even if economically feasible, may damage the community reputation of the company using it, and is clearly not a substitute for high validity in test administration.

It should be emphasized that even unlimited improvement in test validity cannot solve all the problems of industrial selection. This is an important implication of a recent article by Brown and Ghiselli (3) explicating the relations between certain situational variables and production proficiency. According to the accompanying nomograph, given a test which predicts perfectly, a situation in which the worst worker selected by previous means showed one-tenth the production of the best, and a selection ratio of 10 per cent, an over-all proficiency increase of only about 58 per cent may be anticipated. This may seem small compared to the gains anticipated by enthusiastic proponents of tests. Nevertheless, working under conditions far less ideal than those cited, it has been possible to demonstrate the utility of tests in economic terms acceptable to the toughest-minded business man. The variance unaccounted for in the present use of tests suggests that they have by no

means reached a limit in their accuracy of prediction. What can be done to reduce further this "error" variance?

The Need for Wider Sampling of Behaviour

When validities are low despite known high reliabilities there may be a suspicion that the tests themselves are not sufficiently representative of the abilities called for on the job and sampled in the criterion measures. In such cases one may attempt to broaden the range of behaviour measured by the tests. But since the first attempt has usually exhausted the best intuitions of the personnel man, this often leads to a strictly empirical or "shotgun" approach.

It may be possible to base one's understanding of the test-sampled behaviour on other than simple (or professional) intuition. The use of factorial analysis has proven useful in clarifying the inter-relations among performances on various tests, but suffers from the inherent tautological defect that types of behaviour not originally sampled in the test battery cannot appear in the factors which emerge. Hence, factorial approaches to selection testing have often been formalizations of the "shotgun" approach. The questions of unique solutions and of identifying the factors isolated have also been unsatisfactory features from the viewpoint of the person attempting to put situational meaning into his industrial predictions. The trend towards the inclusion of criterion measures in factorial matrices (e.g., 6) may help from this point of view, but gives little direction to the search for new areas of behaviour suitable for industrial prediction.

Though factorial studies originated in an attempt to test deductions from a specific theory of brain functioning, the study of human abilities since Spearman has largely been carried out by individuals whose primary interests were statistical rather than behavioural or theoretical. Hence theories of behaviour in this area have come to be secondary to the study of empirical relations among performance scores. While knowledge of this type may be said to involve "understanding," it is understanding of a very restricted sort, since it concerns only performances quantitatively sampled and included in single or interrelated matrices. Beyond these rigid boundaries it does not lend itself to speculations and generalizations. The development of tests, on the other hand, calls for just such extrapolation if it is to tap new and fruitful kinds of test behaviour. Ideally, the constructor of tests would be better served by recourse to a broad theory of human behaviour, solidly based on experimental findings in such fields as learning and perception, and containing provisions for what has been learned of human abilities.

A beginning has already been made in this direction. Ferguson (4, 5;

see also 1) has attempted to "present a generalized theory which draws together within a single conceptual framework the study of human learning and the study of human ability." He proposes that what is spoken of as an ability in conventional psychological usage has reference to performance at some crude limit of learning. Since different environments present vastly different opportunities for practice, and hence for the necessary degree of over-learning, modal abilities should differ from culture to culture, and even from stratum to stratum within a culture. If his several suggestions for testing this viewpoint are followed, and findings confirm it, emphasis may again be focussed on the relation between behavioural theory and industrial prediction. In the long run such a programme should do much to make test-sampling of behaviour for predictive purposes more comprehensive and systematic.

In the immediate future, however, we shall have a situation in which our present conglomeration of abilities, aptitudes, capacities and criterion measures is neither statistically distinct nor conceptually sound. It will not be wise for improvement at the applied level to wait upon the establishment of a comprehensive theoretical foundation. In certain areas it should be possible to arrive at novel and useful types of selection tests by drawing upon accepted (or even speculative) psychological theory, experimental findings and laboratory techniques. It is probable that too much of our effort has been influenced by a combination of the need for a test to do a particular type of selection, the requirement for its development in a minimum time, and conditions which penalize any lack of success which follows the use of unorthodox procedures.

Hypothetical Constructs in Test Construction

When constructing a test or series of tests for a specific job, it is customary to base it upon a comprehensive analysis of the behaviour apparently involved and called for in job performance. When the work being considered is manual, such a scrutiny of its overt aspects may seem a reasonable basis for determining the types of behaviour which should be sampled. But a job analysis of the work performed by an architect, if carried out in the same way, might contain many such phrases as, "sits staring fixedly at a blank sheet of drawing paper." In such cases the observation of overt behaviour is obviously an inadequate basis for analysing the abilities which the job requires. Hence, from the output of the worker the analyst infers the psychological processes involved, and in this he is aided by verbal reports from the individual. The processes arrived at are hypothetical constructs, and the subsequent basis of test construction is thus conceptually distinct from that involved when data are obtained from direct observation of overt behaviour.

It may be believed that this hypothetical approach is justified only where it is difficult or impossible to observe directly significant portions of job performance. However, psychological investigations have disclosed previously unsuspected complexities and a multiplicity of variables in the simplest types of activities. (Central processes *may* be involved even in classical conditioning.) There may, therefore, be justification for the use of similarly inferred constructs in devising tests for situations in which job analysis has heretofore been considered adequate. The purpose would not be to replace the latter method, but to augment the validity of the tests by a wider sampling of behaviour. At the same time it may be possible to make constructive use of theoretical and speculative concepts developed in other areas of psychology.

Suppose one is interested in the part played by visual stimuli in the uniformity of product turned out by operators of surface finishing machines. Investigation might show that good and poor operators show no reliable differences in their ability to discriminate between pairs of samples finished to different specifications, though both groups can do this with considerable accuracy. Instead of abandoning this avenue of approach, one might consider "visual memory" as a possible relevant variable, on the rationale that good operators are able to maintain a more stable and accurate visual image of the appearance of various qualities of finished work. Through recourse to the literature dealing with visual memory one might estimate optimal conditions, devise a suitable test procedure involving a delay between the presentation of members of a stimulus pair, and determine that it does select the better workers. It might also be possible to decide from the literature whether or not the ability sampled is "trainable," and hence whether one should use this test to select apprentices, or only to screen experienced workers.

With this orientation, a psychologist whose interests are in the applied field is likely to be less "stimulus-bound" in his activities. Given a general awareness of the need for a wider sampling of behaviour to improve industrial prediction, he is likely to scrutinize each new psychological concept in terms of its relevant utility. His viewpoint will not be restricted to procedures already proven in his area of work for the specific task on hand. The resultant broadening of his perceptual horizon should do much to bridge the widening gap between psychology as a theoretical structure and psychology as an applied technology.

Summary

Can psychologists doing applied work in industrial selection claim such a title other than by courtesy? Doubts of this nature might be resolved and accuracy of prediction enhanced by concentration upon the relevant

utility of psychological concepts and their translation into operational terms, rather than upon devising tests to meet specific needs.

Among the factors lowering predictive efficiency is the lack of adequate sampling by present tests of behaviours present in criteria and required on the job. While a comprehensive theory of abilities is desirable and may even be inevitable, more representative tests may in the meantime be derived from the use of hypothetical constructs based upon theory from academic psychology as well as upon "observation and common sense."

REFERENCES

1. Attridge, B. F., & Sampson, H. A note on Ferguson's learning ability matrix. *Canad. J. Psychol.*, 1955, **9**, 84–90.
2. Bingham, W. V. Expectancies. *Educ. psychol. Measmt.*, 1953, **13**, 47–53.
3. Brown, C. W., & Ghiselli, E. E. Per cent increase in proficiency resulting from use of selective devices. *J. appl. Psychol.*, 1953, **37**, 341–344.
4. Ferguson, George A. On learning and human ability. *Canad. J. Psychol.*, 1954, **8**, 95–112.
5. Ferguson, George A. On transfer and the abilities of man. *Canad. J. Psychol.*, 1956, **10**, 121–131.
6. Graham, W. R. Identification and prediction of two training criterion factors. *J. appl. Psychol.*, 1954, **38**, 96–99.
7. Haire, M. Use of tests in employee selection. In Karn, H. W., & Gilmer, B. H. (eds.), *Readings in industrial and business psychology.* New York: McGraw-Hill, 1952.
8. Heron, A. Scientific and professional problems of the psychologist in industry. *Occup. Psychol.*, 1955, **29**, 164–172.
9. Thorndike, R. L. *Personnel selection.* New York: Wiley, 1949.
10. Wallace, S. R., & Weitz, J. Industrial psychology. In Stone, C. P. (ed.), *Annual review of psychology*, vol. 6. Stanford: Annual Reviews Inc., 1955.

APPARENT SLEEP PRODUCED BY CORTICAL STIMULATION[1]

NEAL M. BURNS
McGill University

THE EFFECTS of lesions and stimulation of the cortex on the level of activity and arousal of animals have been studied extensively in recent years. Lashley (**29**) found that injury to the frontal cortex in rats produced increased activity, while lesions in the occipital poles resulted in decreased activity. Richter and Hawkes (**39**) and Beach (**5**) also observed hyperactivity resulting from frontal lesions and, less frequently, a decrease in activity with occipital lesions. Smith (**46**) found that electrical stimulation of the rostral cingulate cortex in monkeys caused cessation of body movements; the eyes closed and a sleep-like state ensued, outlasting the period of stimulation.

The effects of subcortical lesions and stimulation on sleep and waking have also been much investigated. Bremer in 1935 showed that transecting the brain stem at the intercollicular level resulted in a preparation in which the electrocorticogram resembled that usually obtained during sleep. Nauta (**36**) produced deep sleep in rats by placing lesions in the vicinity of the mammillary bodies; lesions involving the lateral hypothalamus gave rise to intermittent spells of drowsiness, but no other subcortical lesions produced somnolence. Hess, Koella and Akert (**24**) were able to produce apparently normal sleep in cats by stimulating either the thalamus or the caudate nucleus. Meyer and Hunter (**35**) reported apathetic and akinetic states as a result of lesions in the anteromedial thalamus in cats and monkeys. Electroencephalographic (EEG) recordings of these animals after stimulation resembled those of normal control animals taken during sleep. A permanent decrease in the activity of rats after bilateral lesions in the amygdaloid nuclei was reported by Anand and Brobeck (**2**). Schreiner, Rioch, Pechtel, and Masserman (**41**), observed "lethargy, somnolence, and motor disability" for several weeks after lesions in the intralaminar nuclei of the thalamus. More recently, Rosvold and Delgado (**40**) were able to put monkeys to sleep by intermittent stimulation of the caudate nucleus or the septal area.

[1]This research was supported by grants from the National Research Council of Canada, the Ford Foundation, and the Foundations Fund for Research in Psychiatry. The writer would also like to acknowledge the assistance of Dr. P. M. Milner during all phases of the study.

In the study to be described, rats were stimulated by electrodes chronically implanted in either the frontal or the occipital cortex.

Methods

Subjects

The subjects were 30 hooded rats from the Royal Victoria Hospital colony. They weighed between 200 and 225 grams each at the time of operation.

Operative and Anatomical Procedures

Bipolar electrodes consisting of two strands of enameled nichrome wire 0.005 inches in diameter, bare only at the tip, and separated by about ½ mm., were permanently implanted in the manner described by Olds and Milner (37). A recovery period of two to four days was allowed before the animals were stimulated.

On completion of the testing schedule, each animal was perfused with physiological saline, followed by 10 per cent formalin solution. The brains were removed and, after fixing for several days, frozen sections were cut at 40 micra and stained with cresyl violet to determine the depth and position of the electrode tips.

Because the short electrode wires did not always penetrate the dura, or because of accidental damage to the cortex during removal of the brain, the electrode track could not always be identified in the sections. In these cases it was possible to make a satisfactory estimate of the location by reference to (*a*) sketches of the brains made at the time of removal, (*b*) the Krieg co-ordinates at which the electrode was implanted, and (*c*) the length of the electrode wires. The placements are marked on a dorsal view of the cortex in Figure 1.

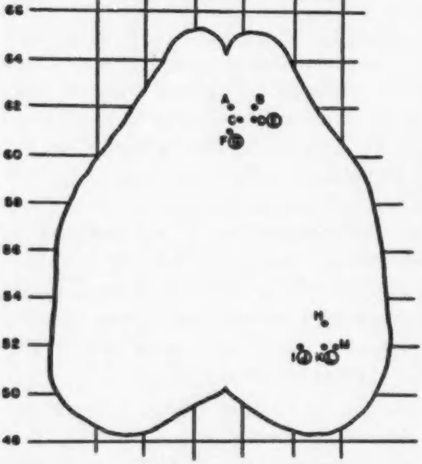

Figure 1. Placements of electrodes for 26 rats. Uncircled letters represent placements histologically determined; circled letters, placements determined by gross examination. Placements for rats 54 and 45, whose records are shown later, were at points M and C, respectively. (Scale: one centimetre to one millimetre.) N.B. Each dot represents placement for one or more rats.

Apparatus

The animals were stimulated in an activity cage similar to that described by Campbell (11). Essentially this is a wire-mesh cylinder 10 inches in diameter and

11⅜ inches high, open at the top. It rests on a central pivot with four microswitches equally spaced round the circumference. These switches open and close as the rat moves about the cage and activate electrical counters.

The stimulation consisted of half-second 5-volt bursts of 60-cycle sine-wave provided by a transformer from the power line. During the stimulation periods the bursts were delivered to the subject at 4-second intervals, the timing being performed automatically by a circuit containing a Hunter timer (Model 111A) and an Agastat time-delay relay. The current reached the rat through a flexible lead suspended from the ceiling and clipped to the electrode.

To prevent the rat from hearing the noise and clicks of the timer, this apparatus was located in a room adjacent to the testing room, and all observations of the animal during testing were made from this room.

Procedure

Each rat was placed in the activity cage for half an hour each day for 6 days. Counter readings were taken after the first 10 minutes and every 5 minutes thereafter. At the beginning of each session 10 minutes were allowed for adaptation, and activity for this period was not measured. Each rat received stimulation on alternate days and served as its own control on non-stimulation days. Half of the animals (group A) were stimulated on their second, fourth and sixth testing days, the remainder (group B) on their first, third and fifth testing days. No stimulation was given during the 10-minute adaptation period; after this, on the appropriate days, the stimulating circuit was turned on for the first and third 5-minute intervals. All the activity measures were taken between 5:00 P.M. and 3:00 A.M., because it was thought that rats, being essentially nocturnal, would be less likely to sleep spontaneously between these hours (44). No systematic attempt was made to equate the times of testing of the rats in different groups, but subsequent statistical examination showed that no significant bias had been introduced.

Affective Quality of Stimulation

In order to determine whether the stimulation had any reinforcing effects (37, 38), the animals were given the opportunity to stimulate themselves in a Skinner box. Eight non-operated animals served as controls to establish the operant level of bar pressing. A t-test for uncorrelated means indicated that the differences in response rates were not significant. Thus the cortical stimulation used in this experiment was what Olds (38) describes as "neutrally reinforcing," that is, the rat learned neither to avoid nor to seek the stimulation.

Results and Discussion

The most striking effect of the stimulation is the decrease in activity it produced in the rats with occipitally located electrodes. As seen in Table I this decrease is significant at the 0.01 level. Equally interesting is the negligible effect of stimulation on the activity of the group with frontal electrodes; the trend is in the same direction as in the occipital animals, but the difference in activity between stimulated and non-stimulated days is not statistically significant. Although activity decreases in both groups on stimulation days, the decrease for the occipital group is very significantly greater than that for the frontal group ($p < .001$).

TABLE I

GROUP DIFFERENCES IN ACTIVITY ON STIMULATED AND NON-STIMULATED DAYS

Group			Activity on stim. days	Activity on non-stim. days	Difference	p
Occipital	(A & B)	$N = 18$	701.7	1129.2	427.5	<.01
Occipital	A	$N = 10$	275.0	436.3	161.3	<.001
Occipital	B	$N = 8$	426.7	692.9	266.2	<.01
Frontal	(A & B)	$N = 8$	151.5	181.6	30.1	>.9
Frontal	A	$N = 4$	78.4	92.1	13.7	>.50
Frontal	B	$N = 4$	73.1	89.5	16.4	>.20

The objective measures of activity facilitated statistical analysis, but they told us little about the state of the animal. However, by watching the rats in the testing situation it was clear that in general the decreased activity, when it occurred, was due to the fact that the rat had fallen into a sleep-like state.

Hess has suggested several criteria by which *true* sleep can be differentiated from other inactive states (**16**): (*a*) reversibility, that is, the ability to arouse a sleeping subject by means of strong sensory stimuli; (*b*) postural characteristics of the species in question; (*c*) certain autonomic correlates, for example, decreased cardio-respiratory output; and (*d*) a typical sleep pattern in the EEG.

In the present investigation, systematic observations with regard to Hess's criteria were not carried out. Incidental observations indicated that sudden noises in the room would usually arouse the subject from its inert state, and that after stimulation the animal usually assumed a typical sleep position. No autonomic measures were taken, but after the formal testing of activity EEG recordings were made on fourteen of the animals (seven frontals and seven occipitals) to determine whether the state induced by stimulation was electrographically different from normal sleep. An Offner type 144A crystograph was employed, recording from the same electrode that was used for stimulation. The rat was placed in a grounded wire-mesh cage $18 \times 14 \times 19$ inches, and records were taken under three conditions: awake, during normal sleep, and as soon as possible after stimulation. Figures 2 and 3 are examples of the records obtained.

In some respects the post-stimulation record resembles spreading depression (**17, 18, 30, 31, 32, 47, 48, 49**). As may be seen from the EEG tracings, there is an almost complete absence of spontaneous activity in the record just after stimulation. (Behaviourally the rat has curled up

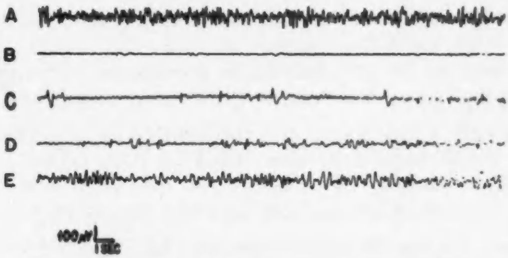

FIGURE 2. Rat No. 54. Electrodes in occipital cortex. *Paper speed:* 1 cm. per second. A, Animal in normal sleep, with record showing activity at 6 per second; B, 1 minute after a 5-minute stimulation period, during which bursts of ½ second 5-volt r.m.s. 60-cycle sine-wave were delivered every 4 seconds—record very flat, animal asleep; C, 4 minutes after stimulation—spike-like bursts begin, animal still apparently asleep; D, 4.5 minutes after stimulation—spikes increasing in frequency, animal remains asleep; E, 9 minutes after stimulation—activity now at 3–4 per second, animal still asleep.

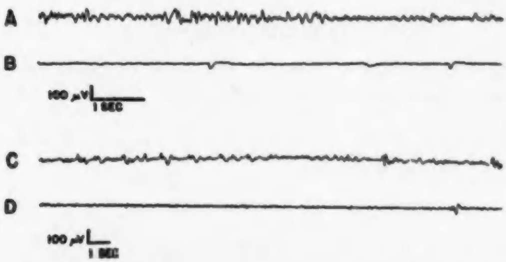

FIGURE 3. Rat No. 45. Electrodes in frontal cortex. *Paper speed:* A & B, 2.5 cm. per sec.; C & D, 1 cm. per sec. A, Animal asleep, activity at 6 per second; B, 30 seconds after animal stimulated as in Figure 2B—record flat, animal asleep; C, 3 minutes after stimulation—occasional spikes and fast activity, animal remains asleep; D, 5 minutes after stimulation—fast activity with very few spikes, animal still asleep.

and appears asleep, although the record is not one of normal sleep.) Approximately four minutes after stimulation (Figure 2) smaller voltages, about 50 μv in amplitude, appear in spike-like bursts. These bursts gradually increase to pre-stimulation amplitudes, although the wave frequency is less.

To identify the phenomenon observed in this experiment as spreading depression would require a more elaborate EEG technique than that employed. Nevertheless, the cessation of spontaneous cortical activity following a period of stimulation, and the timing and manner of its reappearance, are consistent with the idea that spreading depression is involved. However, few, if any, studies of spreading depression have been performed using intact, unanaesthetized animals, and the literature does not appear to mention its being accompanied by sleep. The present results suggest that, if an effect such as spreading depression is involved, it may serve only to initiate the behavioural changes observed.

Several further lines of investigation are suggested by the results of this experiment. Recording and stimulation could be done simultaneously to determine whether spreading depression was present. Simultaneous recordings from subcortical levels might also indicate which structures and circuits were involved in the sleep-like state induced. Isolating the cortex under the stimulating electrode would also aid in determining whether the effect is cortico-cortical in nature or involves subcortical networks.

Theories of Sleep

Several theoretical explanations of the type of phenomenon we have been discussing have been suggested. The following discussion will

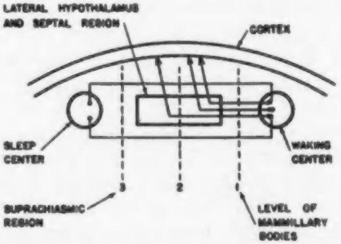

FIGURE 4. Schematic diagram of sleep-walking centres (modified from Nauta, 1946).

review briefly some of these models and the experimental evidence for them, with the present results in mind.

Nauta (36) presented a theory of sleep in which separate sleep and

waking "centres" were postulated. He found that bilateral lesions in the vicinity of the mammillary bodies were capable of producing sleep in rats, and that a state of drowsiness resulted from lesions involving the lateral hypothalamus. Further experiments also showed that lesions in the suprachiasmic region caused sleeplessness. Nauta illustrated with a schematic representation (shown slightly modified in Figure 4) the sort of mechanism he believed to operate in sleep and waking. Lesions at level 1 interrupt the path of impulses from the waking centre to the cortex and thus produce sleep. Lesions at level 2 sever some corticopetal fibres, but leave many intact; this produces chronic drowsiness. Lesions in the suprachiasmic region (level 3) interrupt fibres that inhibit the waking centre and cause sleeplessness. Lindsley, Schreiner, Knowles and Magoun (34) and Meyer and Hunter (35) have substantiated some of Nauta's work by observing drowsiness in cats with lesions in the antero-medial thalamus and the posterior hypothalamus. Bernhaut, Gellhorn and Rasmussen (6) found that sensory stimuli in an intact unanaesthetized animal elicited a general cortical reaction characterized by bilateral activation. They also observed a simultaneous activation of the posterior hypothalamic nuclei and the mammillo-thalamic fasciculus. This finding supports Nauta's postulation of a waking centre in that area.

In contrast to the above postulate of separate sleep and waking centres, Bremer (8, 9) presents a theory of sleep which takes into account neuronal fatigue and cortico-subcortical interplay. He believes electrical stimulation of the diencephalon and telencephalon suppresses the input of the ascending reticular activating system (RAS), thus disturbing the "dynamogenic" effect of the activating system. The process of falling asleep is initiated by a loss of facilitation resulting from neuronal fatigue at cortical, diencephalic and reticular levels. In the normal course of events

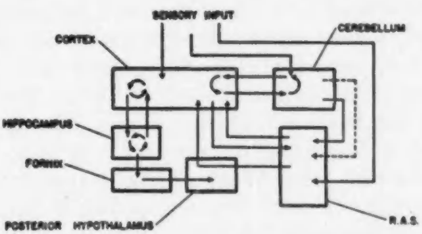

FIGURE 5. Schematic diagram of major components involved in sleep and waking.

this process is cumulative ("*en avalanche*") and is favoured by a decreased sensory input; for example, the initial de-activation might lead to a cessation of motor activity and be followed by a closing of the eyes,

which would progressively decrease proprioceptive and general stimulus inputs (13, 27, 28, 45).

The results of the experiment reported here, together with other recent work, suggest a new interpretive framework concerning sleep mechanisms. Those components of the central nervous system which, it is suggested, participate in the sleep-waking cycle are shown schematically in Figure 5. The connections of the RAS with the posterior hypothalamus have long been regarded as important for arousal and sleep. Lesions in this area have been followed by synchronization in the EEG and correlated with behavioural changes indicating little activation (10, 15, 33). Bremer (9) and Segundo, Naquet, and Buser (43) have observed cortical desynchronization (arousal) to result from stimulation of those cortical areas which send fibres to the reticular system. Recently Hebb (23) has postulated that the pathways from the cortex to this system have to do with the central control of arousal, vigilance and attention. When a particular aspect of the environment demands attention, a system such as that just described would seem capable of exerting a depressing or arousing effect in accordance with the momentary requirements of the stimulus situation.

Thus it is possible that the cortical stimulation which produced sleep in the present experiment did so partly by setting up a net inhibitory effect in the RAS. Another possibility is that several minutes of intense activity caused by the stimulation may fatigue the cortical cells and those to which they connect in the RAS. Their facilitatory effect on the RAS would then be removed. In some ways this resembles the suggestion of Gualtierotti, Martini, and Marzorati (21, 22), who postulated that a sort of cortical exhaustion followed diathermic destruction of the thalamus.

Recent investigations have pointed to the role of the hippocampus in sleep and arousal (25, 26). Inverse patterns of activity in the cortex and hippocampus have been found by Green and Arduini (20). Thus, when the cortex displays slow waves of high amplitude (as in sleep), the hippocampal record reveals fast irregular waves. Studies of the connections to and from the hippocampus have shown that communication exists via the fornix to the mammillary bodies, mammillo-thalamic tract and the anterior thalamus (19). It had been suggested earlier by Green and Arduini that the connections to the mammillary body—Nauta's waking centre—might inhibit the posterior hypothalamic priming zone and aid in the onset of sleep. Also relevant at this point is the sleep-like state induced in man by stimulation of the fornix (42), and the work of Brady and Nauta (7), in which rats with lesions in the supracommissural part of the septal area and most of the fornix exhibited an unusual degree of alertness and hyper-responsiveness.

The manner in which these data fit into the theoretical schema pro-

posed here (Figure 5) is, in part, a modification of Nauta's formulation (Figure 4). The sleeplessness which he observed to follow lesions in the suprachiasmic region, and which led him to postulate the existence of a sleep centre in that area, is assumed here to be the result of lesions involving the hippocampal-posterior hypothalamic pathway. Corticofugal tracts to the hippocampus (e.g., tempero-ammonic) might then be assumed to initiate the firing of this inhibiting network. However, the work of Bard and Rioch (3), Allen (1), and Bard and Mountcastle (4) has indicated that removal of the hippocampus produces very little change in sleep patterns; this is difficult to explain on the present hypothesis.

Regardless of the arguments for and against inhibitory processes at a central level (12, 14), this experiment has demonstrated the behavioural consequences of interfering with the normal function of one component of a complex system, and re-emphasizes the importance of the cortex in the maintenance of the waking state.

Summary

Cortical electrodes were chronically implanted in the brains of thirty rats, and the behavioural and EEG effects of electrical stimulation were investigated. It was found that a condition closely similar to sleep followed periods of stimulation in the occipital cortex, whereas stimulation of the frontal cortex had no significant effect on the animals' activity. The results are interpreted in terms of a postulated system of neural connections necessary for maintenance of the sleep-waking cycle.

REFERENCES

1. Allen, W. F. Effect of ablating the pyriform amygdaloid areas and hippocampi on positive and negative olfactory conditioned reflexes and on conditioned olfactory differentiation. *Amer. J. Physiol.*, 1941, **132**, 81–92.
2. Anand, B. K., & Brobeck, J. R. Food intake and spontaneous activity of rats with lesions in the amygdaloid nuclei. *J. Neurophysiol.*, 1952, **15**, 421–430.
3. Bard, P., & Rioch, D. McK. A study of four cats deprived of neocortex and additional parts of the forebrain. *Bull. Johns Hopkins Hosp.*, 1937, **60**, 63–147, cited by Green and Arduini (1954).
4. Bard, P., & Mountcastle, V. B. Some forebrain mechanisms involved in expression of rage with special reference to suppression of angry behaviour. *Res. Publ., Ass. nerv. ment. Dis.*, 1948, **27**, 362–404.
5. Beach, F. A. Effects of brain lesions upon running activity in the male rat. *J. comp. Psychol.*, 1941, **31**, 145–179.
6. Bernhaut, M., Gellhorn, E., & Rasmussen, A. T. Experimental contributions to problem of consciousness. *J. Neurophysiol.*, 1953, **16**, 21–35.
7. Brady, J. V., & Nauta, W. J. H. Subcortical mechanisms in emotional behavior: Affective changes following septal forebrain lesions in the albino rat. *J. comp. physiol. Psychol.*, 1953, **46**, 339–346.
8. Bremer, F. *Some problems in neurophysiology*. London: Athlone Press, 1953.

9. Bremer, F. The neurophysiological problem of sleep. In Adrian, E. D., Bremer, F., & Jasper, H. H. (eds.), *Brain mechanisms and consciousness*, pp. 37–162. Oxford: Blackwell, 1954.
10. Brodal, A., & Rossi, G. F. Ascending fibers in brain stem reticular formation of cat. *Arch. Neurol. Psychiat.*, 1955, **74**, 68–87.
11. Campbell, B. A. Design and reliability of a new activity-recording device. *J. comp. physiol. Psychol.*, 1954, **47**, 90–92.
12. Clark, G. Suppression and facilitation: A review. *Quart. Chicago Med. Sch.*, 1944, **10**, 14–26.
13. Coriat, I. H. The nature of sleep. *J. abnorm. Psychol.*, 1912, **6**, 329–367.
14. Dusser de Barenne, J. G., & McCulloch, W. S. Suppression of motor response upon stimulation of area 4–s of the cerebral cortex. *Amer. J. Physiol.*, 1939, **126**, 482 (Abstract).
15. French, J. D., von Amerongen, F. K., & Magoun, H. W. An activating system in brain stem of monkey. *Arch. Neurol. Psychiat.*, 1952, **68**, 577–590.
16. Gloor, P. Autonomic functions of the diencephalon: A summary of the experimental work of Prof. W. R. Hess. *Arch. Neurol. Psychiat.*, 1954, **71**, 773–790.
17. Grafstein, B. Mechanisms of spreading cortical depression. *J. Neurophysiol.*, 1956a, **19**, 154–171.
18. Grafstein, B. Locus of propagation of spreading cortical depression. *J. Neurophysiol.*, 1956b, **19**, 308–316.
19. Green, J. D., & Adey, W. R. Electrophysiological studies of hippocampal connections and excitability. *EEG clin. Neuro-physiol.*, 1956, **8**, 245–262.
20. Green, J. D., & Arduini, A. A. Hippocampal electrical activity in arousal. *J. Neurophysiol.*, 1954, **17**, 533–557.
21. Gualtierotti, T., Martini, E., & Marzorati, A. Electronarcosis. III. Inhibition of cortical electrical activity following local application of pulsed stimulus. *J. Neurophysiol.*, 1950a, **13**, 5–8.
22. Gualtierotti, T., Martini, E., & Marzorati, A. Electronarcosis. V. Faradic stimulation of motor area following diencephalic diathermy. *J. Neurophysiol.*, 1950b, **13**, 117–126.
23. Hebb, D. O. Drives and the C.N.S. (conceptual nervous system). *Psychol. Rev.* 1955, **62**, 243–254.
24. Hess, R., Jr., Koella, W. P., & Akert, K. Cortical and subcortical recordings in artificially induced sleep in cats. *EEG clin. Neurophysiol.*, 1953, **5**, 75–90.
25. Hunter, J. Further observations on subcortically induced epileptic attacks in unanaesthetized animals. *EEG clin. Neurophysiol.*, 1950, **2**, 193–201.
26. Kaada, B. R., & Jasper, H. Respiratory responses to stimulation of temporal pole, insular and hippocampal and limbric gyri in man. *Arch. Neurol. Psychiat.*, 1952, **68**, 609–619.
27. Kleitman, N. Studies on the physiology of sleep, I. *Amer. J. Physiol.*, 1923, **66**, 67–92.
28. Kleitman, N. *Sleep and wakefulness.* Chicago: Univer. of Chicago Press, 1939.
29. Lashley, K. S. Studies of cerebral function in learning. *Psychobiol.*, 1920, **2**, 55–135.
30. Leao, A. A. P. Spreading depression of activity in the cerebral cortex. *J. Neurophysiol.*, 1944, **7**, 359–390.
31. Leao, A. A. P. Further observations on the spreading depression of activity in the cerebral cortex. *J. Neurophysiol.*, 1947, **10**, 409–414.

32. Leao, A. A. P., & Morison, R. R. Propagation of spreading cortical depression. *J. Neurophysiol.*, 1945, **8**, 33–45.
33. Lindsley, D. B., Bowden, J., & Magoun, H. W. Effect upon EEG of acute injury to the brain stem activating system. *EEG clin. Neurophysiol.*, 1949, **1**, 475–486.
34. Lindsley, D. B., Schreiner, L. H., Knowles, W. B., & Magoun, H. W. Behavioral and EEG changes following chronic brain stem lesions in the cat. *EEG clin. Neurophysiol.*, 1950, **2**, 483–498.
35. Meyer, J. S., & Hunter, J. Behavior deficits following diencephalic lesions. *Neurol.*, 1952, **2**, 112–130.
36. Nauta, W. J. H. Hypothalamic regulation of sleep in rats: An experimental study. *J. Neurophysiol.*, 1946, **9**, 285–316.
37. Olds, J., & Milner, P. M. Positive reinforcement produced by electrical stimulation of septal area and other regions of rat brain. *J. comp. physiol. Psychol.*, 1954, **47**, 419–427.
38. Olds, J. A preliminary mapping of electrical reinforcing effects in the rat brain. *J. comp. physiol. Psychol.*, 1956, **49**, 281–285.
39. Richter, C. P., & Hawkes, C. D. Increased spontaneous activity and food intake produced in rats by removal of the frontal poles of the brain. *J. neurol. Psychiat.*, 1939, **2**, 231–242.
40. Rosvold, H. E., & Delgado, J. M. R. Frontal lobe stimulation and delayed alternation. *J. comp. physiol. Psychol.*, 1956, **49**, 365–372.
41. Schreiner, L., Rioch, D. McK., Pechtel, C., & Masserman, J. H. Behavioral changes following thalamic injury in cats. *J. Neurophysiol.*, 1953, **16**, 234–246.
42. Segundo, J. P., Arana, R., Migliaro, E., Villar, J. E., Garcia Guelfi, A., & E. Garcia Austt, H. Respiratory responses from fornix and wall of third ventrical in man. *J. Neurophysiol.*, 1955, **18**, 96–101.
43. Segundo, J. P., Naquet, R., & Buser, P. Effects of cortical stimulation on electrocortical activity in monkeys. *J. Neurophysiol.*, 1955, **18**, 236–245.
44. Shirley, M. Studies in activity. II. Activity rhythms: Age and activity; activity after rest. *J. comp. Psychol.*, 1928, **8**, 159–186.
45. Sidis, B. An experimental study of sleep. *J. abnorm. Psychol.*, 1908, **3**, 1–32; 63–96; 170–207.
46. Smith, W. K. The functional significance of the rostral cingular cortex as revealed by its responses to electrical excitation. *J. Neurophysiol.*, 1945, **8**, 241–255.
47. van Harreveld, A., & Stamm, J. S. Spreading cortical convulsions and depressions. *J. Neurophysiol.*, 1953, **16**, 352–366.
48. van Harreveld, A., & Stamm, J. S. Consequences of cortical convulsive activity in rabbit. *J. Neurophysiol.*, 1954, **17**, 505–520.
49. Winokur, G. L., Trufaut, S. A., King, R. B., & O'Leary, J. L. Thalamocortical activity during spreading depression. *EEG clin. Neurophysiol.*, 1950, **2**, 79–90.

EFFECT OF GLUTAMIC ACID ON THE LEARNING ABILITY OF BRIGHT AND DULL RATS: II. DURATION OF THE EFFECT[1]

K. R. HUGHES AND JOHN P. ZUBEK
University of Manitoba

IN AN EARLIER PAPER, Hughes and Zubek (1) reported that the feeding of glutamic acid to a strain of dull rats (McGill) resulted in a considerable improvement in maze-learning ability. A similar improvement was not obesrved in a strain of bright rats treated in the same manner. In view of the significant improvement in learning ability, the question arises as to whether the effect is a permanent one or is dependent upon the continued administration of the glutamate supplement. The present study was designed to answer this question.

PROCEDURE

Subjects

A group of 24 rats of the McGill dull strain (F_{11}), which had served as subjects in a previous study (1) demonstrating the effect of glutamic acid on learning ability, were used in the experiment. They were divided into an experimental group containing 13 rats (7 males, 6 females), and a control group containing 11 rats (5 males, 6 females).

Apparatus

The 12 problems of the Hebb-Williams closed-field maze were administered in the manner described by Rabinovitch and Rosvold (2).

Procedure

In the original experiment (1) the animals were weaned at 25 days of age and immediately placed on an experimental feeding schedule for 40 days. During this time the animals of the experimental group received a daily supplement of 200 mg. monosodium glutamate while the controls were given a placebo. At 65 days of age the supplementary feeding was discontinued and the animals were tested on the Hebb-Williams maze (Test I).

Thirty days after the completion of the maze problems and discontinuance of the glutamate diet, the animals were again placed on the training schedule of the Hebb-Williams maze and retested, this time on the mirror-image form of the original problems (Retest I). Sixty days after the completion of Retest I, the animals were placed on the training schedule for a third time and retested on the original problems (Retest II). Error scores were recorded in each testing session, and a total score for each animal, based on its performance on the three tests combined, was also calculated.

[1]This research was supported by a grant-in-aid from the Associate Committee on Applied Psychology of the National Research Council of Canada.

Results

The mean error scores obtained by the experimental and control animals on each of the tests are shown in Table I. As might be expected, both groups of animals show a reduction in error scores upon relearning the mirror-image form of the maze in Retest I, and a further reduction when retested for a second time. This undoubtedly is due to the effects of practice. However, the significant fact is that in the original experiment as well as in the two retests the glutamate-fed animals made fewer errors than the controls. In the first retest, 30 days after the discontinuation of glutamic acid, they made 16.3 fewer errors than the controls. This difference is statistically significant ($t = 3.51$, $p < .05$). In the second retest, carried out 90 days after discontinuation of the special diet, the experimental animals again made fewer errors than the controls. This difference, however, only borders on significance ($t = 1.66$, $p = .10$). The smallness of this difference is undoubtedly due to the fact that repeated practice on the maze problems had brought the animals to a level of performance beyond which there was little room for improvement. At this high level of performance the effects of glutamic acid would not be expected to show up.

TABLE I

Test-Retest Error Scores Obtained by Glutamate-Fed and Control Animals

	Mean error score		Mean difference	p
	Glutamate	Control		
Test I	127.5	164.0	36.5	.01
Retest I	62.1	78.4	16.3	$> .01 < .05$
Retest II	58.3	68.2	9.9	.10
3-test total	247.9	310.6	62.7	.01

Discussion

The results of the present experiment indicate that a difference between the glutamate-fed and control dull rats can still be observed in retests carried out one month and three months after the termination of the glutamic acid supplementary feeding. These findings are in line with those of Zimmerman and Burgemeister (3), who reported that a group of mentally retarded children still showed an intellectual gain two years after the cessation of glutamate feeding. It would appear, therefore, that the effects of glutamic acid on maze learning and intellectual processes can be relatively permanent.

Summary

The purpose of the present experiment was to study the duration of improved learning ability brought about by supplementary feeding of glutamic acid to a strain of dull rats.

In a previous experiment (1) 24 dull rats had been weaned at 25 days of age and placed on a 40-day feeding schedule in which the experimental animals were given a daily supplement of 200 mg. monosodium glutamate and the control animals a placebo. At the end of this time the supplement was discontinued and the animals were tested on the Hebb-Williams maze. Thirty days after the completion of this test, the animals were retested on the mirror-image form of the original maze problems, and sixty days after this retest the animals were again tested on the original problems.

The results indicated that the feeding of monosodium glutamate could improve the learning ability of dull rats, and that this improvement was still evident over a period of three months after cessation of the dietary supplement.

REFERENCES

1. Hughes, K. R., & Zubek, J. P. Effect of glutamic acid on the learning ability of bright and dull rats. I. Administration during infancy. *Canad. J. Psychol.*, 1956, **10**, 132–138.
2. Rabinovitch, M. S., & Rosvold, H. E. A closed field intelligence test for rats. *Canad. J. Psychol.*, 1951, **5**, 122–128.
3. Zimmerman, F. T., & Burgemeister, Bessie B. Permanency of glutamic acid treatment. *Arch. Neurol. Psychiat.*, 1951, **65**, 291–298.

MAINTENANCE OF AVOIDANCE BEHAVIOUR WITH INTERMITTENT SHOCKS

JOHN J. BOREN AND MURRAY SIDMAN
Walter Reed Army Institute of Research, Washington, D.C.

IN EXPERIMENTS ON AVOIDANCE BEHAVIOUR the avoidance response generally prevents the occurrence of a shock. If the organism fails to make the avoidance response, the shock inevitably follows within a specified time. The present experiment investigates the effects of omitting a certain proportion of the shocks "due" an animal when it has failed to make the avoidance response.

In the procedure employed here, no warning stimulus was presented, but every time the lever was pressed the shock was postponed for a given period of time. If the animal waited 20 seconds without pressing the lever, a shock became "due." Our independent variable was the proportion of such "due" shocks that was actually delivered.

METHOD

Subjects and Apparatus

Subjects were 5 male albino rats. The apparatus, described elsewhere (2), consisted of a small chamber with a lever at one end and a grid floor through which electric shock could be administered. Responses and shocks were recorded on magnetic counters. Recording and programming were accomplished automatically by relays, timers, and associated apparatus.

Procedure

The animals were maintained on an *ad lib.* feeding and watering schedule in their home cages. During the experimental sessions, however, no nutrients were provided. Sessions lasted 3 hours for animals SD-51 and GF-12, and 6 hours for the remaining 3 animals.

The lever-pressing response was first conditioned by means of a free-operant avoidance technique in which no exteroceptive warning stimulus was employed (1). A shock was delivered every 20 seconds if no response occurred (shock-shock interval). Every time the lever was pressed, however, the shock was postponed for 20 seconds (response-shock interval). Only the downward movement of the lever postponed the shock; continued holding of the lever had no effect upon shock delivery. The shock was of brief, fixed duration (approximately 0.3 sec.).

All animals had a hundred or more hours of experience on this avoidance procedure and their response rates were stable (displaying no consistent trends) before the intermittent shock procedure was introduced.

In the 100 per cent shock procedure, the animals received a shock every time 20 seconds elapsed without their pressing the lever. In the intermittent shock procedures, the animals received a shock only on a predetermined percentage of the occasions on which they waited 20 seconds without responding. For example, in the

CANAD. J. PSYCHOL., 1957, 11 (3)

50 per cent shock procedure a shock became "due" each time the animal waited 20 seconds without pressing the lever, but only 50 per cent of the "due" shocks were actually delivered. The sequence of omitted and delivered shocks was randomized within each shock percentage.

The shock percentages employed were 100, 50, 30, 20, 10, 5, and 2.5, each percentage being programmed for 7 consecutive sessions. The sequence of shock percentages was the same for all animals, starting with 100 per cent and proceeding in descending serial order until the rate of lever pressing dropped to a near-zero level. After this series, the animals were exposed a second time to selected shock percentages. The response rates thus obtained served as controls for the possible effects of the descending serial order itself.

In order to minimize the influence of "warm-up" time, the first hour of each session was not included in the calculations. The values presented for response rates and number of shocks are the medians of the *last four* sessions on a given shock percentage for each individual subject. The first three sessions were omitted in order to free the measures of maintained avoidance behaviour from transition effects that might result from the change to a new shock percentage.

Results

Shock Probability and Avoidance Responses

The rate of avoidance responding for each animal as a function of the percentage of due shocks actually delivered is presented in Figure 1. From 100 down to 30 per cent all the rates are relatively constant. Below 30 per cent the rates begin to decline, with a sharp, almost discontinuous, break in the curves between the lowest percentages. The more gradual decline shown by rat GF-12 is discussed below. For rat AA-16, technical difficulties prevented continuation below the 5 per cent shock level, but it is apparent that there would have to be a sharp decline in rate between that point and zero, which represents extinction.

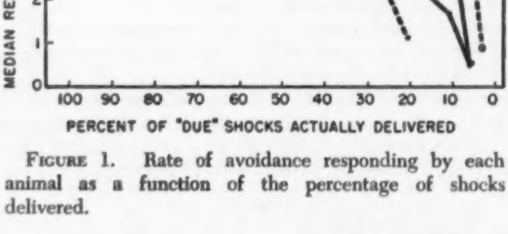

FIGURE 1. Rate of avoidance responding by each animal as a function of the percentage of shocks delivered.

Because the animals were exposed to the shock percentages in descending serial order, the possibility existed that the decline in rate might be simply a function of time, and the relation to shock percentage coincidental. As a direct check on this possibility, one point on the curve for each animal was re-determined. After obtaining the last point on the curves of Figure 1, each animal was returned to the immediately preceding shock schedule. The results of re-determining these rates are presented in Figure 2.

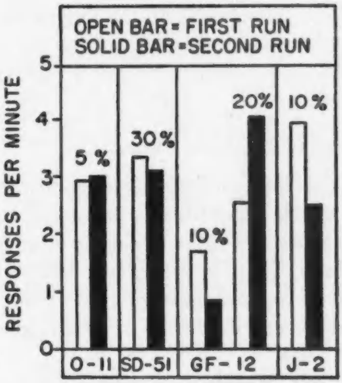

FIGURE 2. Comparison of original and redetermined response rates for each animal at the given shock percentages.

Rat O-11, whose avoidance responding was nearly extinguished at 2.5 per cent shock, was returned to 5 per cent shock. The response rate returned to its previous level for this point, indicating that the drop in rate at 2.5 per cent was a function of shock percentage and not simply of time. The same was true of rat SD-51, which was returned from 20 to 30 per cent shock. Rat GF-12, however, did show some indication of a temporal order effect when returned from 5 to 10 per cent shock, the original response rate not being completely recovered. A second determination was then made at 20 per cent shock, and here the rate not only returned to its original level but also considerably overshot it. Although we are unable to explain this overshooting, it seems evident that rat GF-12 would have shown a sharp drop in rate at 10 per cent shock if it had been permitted more than seven sessions at this value. The control suggests that the curve for GF-12 in Figure 1 is too high at 10 per cent, and that the function is actually consistent with those produced by the

other animals. For rat J-2, the rate is only partially recovered upon return from 5 to 10 per cent shock. This may indicate that the curve for this animal, too, is spuriously high at 10 per cent, or the difference may here be simply a matter of variability, correlated with the sharply changing characteristic of J-2's curve in this region. Curves based on the original and on the re-determined value at 10 per cent shock would be only slightly different, however, and both would be in accord with those of the other animals.

By demonstrating the reversibility of the rate changes as a function of the percentage of due shocks delivered, the data of Figure 2 are an effective control for the effects of serial order. It is unlikely that the curves in Figure 1 can be attributed to long-term extinction or to some other extended process.

Shocks Received and Inter-Response Times

The median number of shocks received by each animal during the last four sessions at each shock percentage is presented in Table I. Three of the animals show a substantial drop in shocks received from the 100 to the 50 per cent schedules. There appears, however, to be little correlation between shocks received and response rate. With the possible exception of rat GF-12, the number of shocks remains relatively constant at schedules below 50 per cent.

In order to clarify our discussion of the relations between the percentage of due shocks delivered, the rate of avoidance responding, and

TABLE I

MEDIAN NUMBER OF SHOCKS RECEIVED BY EACH ANIMAL AS A FUNCTION OF THE SHOCK PERCENTAGE

Per cent shock	J-2	AA-16	O-11	SD-51	GF-12
100	117	12	24	204	105
50	46	10	24	119	69
30	37	12	15	74	72
				76*	
20	39	13	19	81	58
					38*
10	29	11	13	—	35
	50*				42*
5	37	9	14	—	33
			17*		
2.5	—	—	17	—	—

*Second determination.

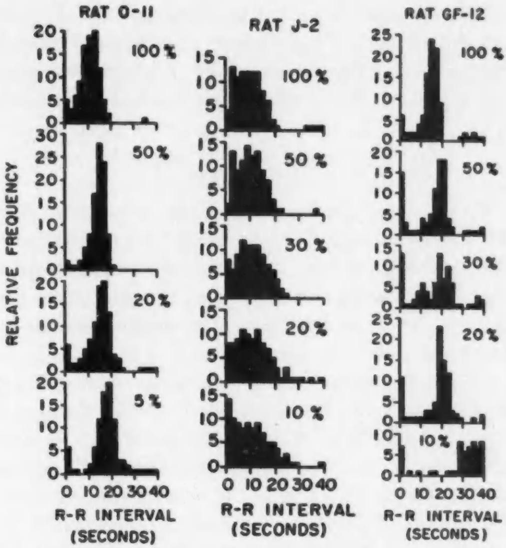

FIGURE 3. Relative frequency distributions of time intervals between successive responses at each shock percentage for rats O-11, J-2, and GF-12.

the number of shocks received, we present the inter-response time data of Figure 3. With three of the animals, frequency distributions of the time intervals between successive bar pressing responses were recorded during the final session at each shock percentage (with the exception of 30 per cent for rat O-11). These distributions provide a measure of the efficiency with which the animals spaced their avoidance responses.

The distributions illustrate individual variations in the magnitude of what has been termed the "Type 2" deviation from random responding (3). These deviations, indicating varying degrees of timing accuracy, are represented by the peaks of the distributions prior to 20 seconds, when the shock was due. The high frequency of rapid "bursts" of responses, especially prominent in rat GF-12, has been observed previously (3), but remains unexplained.

Although there are wide differences in the distributions from animal to animal, they display similar trends as a function of the shock percentage. With decreasing percentages of due shocks received, the peaks of the distributions tend to move slightly toward longer intervals between responses, and there is a small increase in the proportion of inter-response times between 20 and 40 seconds. These changes are small and may not

be reliable. With the exception of the distribution at 10 per cent shock for rat GF-12, the spacing of avoidance responses is remarkably stable with decreasing shock percentages. (The 2.5 per cent results are not shown because response frequencies were too low to produce a reliable distribution.)

Discussion

The most obvious, and perhaps the most important, finding of this experiment is that avoidance behaviour can be maintained with much greater economy of shocks than has hitherto been attempted. As much as 70 per cent of the shocks normally administered when the avoidance response does not occur in time may be omitted without appreciably lowering the response rate.

The effect of the intermittent shock procedure upon the response-shock and shock-shock intervals, however, makes this finding unexpected. Whenever a shock comes due after 20 seconds of no response and is not delivered, the animal receives an additional 20 seconds of "grace" before another shock can be given. Even if the animal presses the lever before the end of the period of grace, it has had the opportunity to "discover" that longer waiting times than 20 seconds are possible without being shocked. If, instead, it waits until the next due shock is delivered, it has an opportunity to experience a response-shock interval considerably longer than 20 seconds. Each due shock not delivered increases the interval another 20 seconds. Previous findings have demonstrated that the rate of avoidance responding is a decreasing function of the shock-shock and response-shock intervals (2), hence it appeared reasonable to expect a decline in response rate corresponding to the decreasing percentage of due shocks delivered.

The problem may be resolved by determining whether the shock contingencies made possible by the intermittent schedule actually established contact with the behaviour. There are two questions here: (*a*) were longer periods than 20 seconds of no responding, not followed by shock, actually experienced by the animals? (*b*) when a shock did occur, how much time had elapsed since the preceding response? were the response-shock intervals often longer than 20 seconds?

The relative frequency distributions of inter-response times provide a direct answer to question (*a*). They show, with one exception, only a small increase in the frequency of inter-response intervals greater than 20 seconds. Furthermore, these increases were of a small magnitude, indicating that while the animals did space their responses further apart at lower shock percentages, the spacing was not nearly sufficient for them to take full advantage of the shock schedules.

A definitive answer to question (*b*) can be provided only by a record of the time intervals between each shock and the preceding avoidance response. Unfortunately, we were not foresighted enough to record these intervals. There is, however, enough indirect evidence to provide a negative answer to the question.

The first evidence is provided by the data of Figure 1. It is clear that the response rate must decline before a significant number of long response-shock intervals could be experienced by the animals. For example, at a low per cent shock value a rat which stopped responding might not be shocked for 40 or 60 seconds, or even longer. However, the animal must wait this long if the response-shock interval is to affect his behaviour, and such long waits are likely only at very low shock percentages, as illustrated by Figure 3. Therefore, as the shock percentage approaches a value at which the rate decreases, a spiralling process is begun. As the rate declines, longer response-shock intervals are experienced, and these in turn lower the rate still further. Hence, relatively sharp breaks occur in the curves of Figure 1.

Confirmation of the above analysis is provided by the distribution for rat GF-12 at 10 per cent shock. It will be recalled that re-determination of the rate at 10 per cent shock for GF-12 suggested that additional sessions at this value would have resulted in almost complete disappearance of avoidance responding. The marked shift in the distribution indicates that the schedule had, indeed, begun to make contact with the behaviour, and that the spiralling process of increasing response-shock intervals and decreasing response rates had already begun at the level of 10 per cent shock.

The relative constancy of the number of shocks *received* as the percentage *delivered* dropped below 50 (Table I) indicates that longer waiting times followed by shock did occur with decreasing shock percentages. But the exact response-shock intervals cannot be inferred from these data. The shape of the inter-response time distributions, however, suggests a low proportion of waiting times of 40 seconds or longer. The number of such intervals that were followed by shocks must have been even smaller. The long response-shock intervals made possible by the intermittent programme could not be realized in behavioural changes until a greater number of responses were spaced in higher multiples of 20 seconds. The data of Table I, therefore, probably reflect an increase in the proportion of shocks received 20 seconds after a response.

We may conclude that the effects of the intermittent shock procedure can be accounted for only by considering *both* the procedural specifications and the state of the behaviour itself. While the procedure sets up

conditions conducive to low rates of avoidance responding, the maintained rate and temporal distribution of the responses do not permit the changed conditions to "take hold" until the shock percentage falls below a critical value.

Summary

Using a free-operant avoidance technique, this study investigated the effects of varying the probability that shock will actually occur when the animal fails to emit the avoidance response. It was found that the rate of avoidance responding remained essentially constant from 100 per cent to 30 per cent shock. At lower shock percentages, the rate dropped sharply. Data on shock frequencies and inter-response times are also analysed, in terms of interactions between response-shock intervals programmed by the intermittent shock schedule and response-shock intervals actually experienced by the animals.

REFERENCES

1. Sidman, M. Avoidance conditioning with brief shock and no exteroceptive warning signal. *Science*, 1953, **118**, 157–158.
2. Sidman, M. Two temporal parameters of the maintenance of avoidance behavior by the white rat. *J. comp. physiol. Psychol.*, 1953, **46**, 253–261.
3. Sidman, M. The temporal distribution of avoidance responses. *J. comp. physiol. Psychol.*, 1954, **47**, 399–402.

SOME EFFECTS OF MORPHINE ON HABIT FUNCTION[1]

H. D. BEACH

Hospital for Mental and Nervous Diseases, St. John's, Newfoundland

A PREVIOUS STUDY (1) demonstrated that rats could be addicted to morphine in the sense that they learned to prefer stimuli associated with the first hour of the drug's effects. Evidence was presented for two kinds of reinforcement in the addiction process: (*a*) drive reduction reinforcement, or the action of morphine in relieving the distressful withdrawal symptoms which develop some hours after the last in a series of doses of the drug; (*b*) "euphoric" reinforcement, some continuing effect like that which human addicts report as intensely pleasant. In rats, the former type of reinforcement gave rise to a habit which survived complete withdrawal of the drug. The habit established with euphoric reinforcement appeared to be more temporary since it did not survive the three weeks of withdrawal.

The question now arises as to the nature of the morphine habit. Hullian theory (2) states that a habit has no response-evoking properties in the absence of a drive. This would imply that the learned responses involved in the morphine-seeking habit would not be manifested except when the organism is in some state of need. The same hypothesis would be implied for a food-seeking habit. Such an inference is reasonable for food-seeking habits, but seems questionable in a habit where the reinforcement involved is some pleasurable or excitatory effect, like the arousal associated with novel stimuli (3), the "pleasant" stimulation of saccharine (4), or the "euphoria" produced by morphine (1) or cocaine (9). In the following experiment the morphine habit was compared with a food habit under various conditions of deprivation and satiation.

EXPERIMENT I

Method

Subjects were male hooded rats from the Royal Victoria Hospital colony, approximately 100 days old when acquired for experimentation.

[1]This research was done at the McGill University Psychological Laboratory and was supported by grants to Dr. D. O. Hebb from the Rockefeller Foundation and the Foundations Fund for Research in Psychiatry. The writer gratefully acknowledges the aid of K. C. Hossick, Chief of Division of Narcotic Control, Department of National Health and Welfare, and Dr. R. L. Wolfe, Medical Director, Hoffman-La Roche, in the procural of drugs.

CANAD. J. PSYCHOL., 1957, 11 (3)

Of the rats subjected to addiction training, some were those reported previously (1). Since these had been tested under certain of the test conditions of this experiment, they are included where appropriate in this report, thus giving groups with differing numbers of subjects. All the morphine-habit rats were dependent on the drug during addiction training, so that they presumably experienced relief from withdrawal distress, or drive reduction, when given a morphine injection.

The addiction training was carried out in a Y-choice discrimination box (1). At the ends of the arms of the Y were goal boxes differing in colour, shape, and other details. There were 16 pre-training choice trials given in blocks of 4 each, with 12 hours between blocks. Addiction training took place over a period of 12 days and the following daily procedure was carried out: the rats were (1) injected with physiological saline solution in their "living" room; (2) taken to experimental room, run into their *preferred* goal box (as determined from pre-training trials), and left there in groups of from 4 to 6 for one hour; (3) taken to "living" room and injected with regular dose of morphine; (4) run into their *non-preferred* goal box in the experimental room, and left in groups of from 4 to 6 for one hour; (5) removed to home cages in "living" room.

Post-training choice tests in blocks of 4 trials each were carried out under 3 different conditions: (1) when the rats had been without morphine for 24 hours and were presumably "needing" the drug; (2) 2 hours after the rats had received their regular injection of morphine and had spent the following hour in the morphine goal box of the apparatus; (3) 2 hours after the rats had received their regular dose of morphine and had remained in their home cages for the following 2 hours. Rats were tested under conditions (1) and (2) on the first day, under condition (3) on the second day, with similar repeat tests on the third and fourth days.

A food-seeking habit was established in another group of 23 rats, using the same Y-choice discrimination apparatus and a training routine like that used for addiction training. In brief, the rats were adapted for 5 days to an 11½-hour food deprivation schedule with 2 half-hour feeding periods per day, being fed to satiation with wet mash (approximately 13 grams of Purina meal plus 0.5 grams skim milk powder per rat) on a feeding stand. They were then adapted to the apparatus by being placed in it in groups of 6 one hour per day for 3 days. This was followed by 16 pre-training choice trials in blocks of 4 trials each. Blocks of trials were about 12 hours apart, the rats being run in rotation within a given block so that there was 8 to 10 minutes between trials for each rat. The rat was left for 5 seconds in the goal box it entered. No retracing was permitted. No food was available in goal boxes during adaptation or choice trials.

Training covered 6 days with 2 training periods daily, about 12 hours apart. In each period the routine was: (1) run into *preferred* goal box and left in a group of 6 for one half-hour; (2) removed to home cages for 5 to 10 minutes; (3) run into *non-preferred* goal box in a group of 6; (4) after 5 to 7 minutes, given their usual meal of wet mash in this *non-preferred* goal box; (5) kept in this "food" goal box for a total of one half-hour, then removed to home cages in "living" room.

Post-training choice tests in blocks of 4 trials each were carried out under 3 conditions: (1) when rats were food-deprived for 12 hours; (2) 2 hours after rats had been run into the food goal box of the apparatus and fed there as in training; (3) 2 hours after being fed to satiation in their home cages and left there for 2 hours. Rats were tested under conditions (1) and (2) on the first post-training test day, under condition (3) on the second day, with similar repeat tests on the third and fourth days respectively.

Treatment of Data

In both pre-training and post-training tests each rat's choice on the *first* trial of a 4-trial block was recorded, as such *first* choices probably indicate the animal's initial differential reactivity to the 2 goal boxes. *First* choices are reported as ratios, with the number of rats choosing the rewarded goal box over the number choosing the non-rewarded goal box, the rewarded goal box being the morphine goal box for morphine-habit rats and the food goal box for food-habit rats. In addition, *total* choices in the 2 test blocks (8 trials) were recorded as a probable measure of how well a particular choice persisted. These are reported as means of the percentages of the 8 trials on which rats chose the rewarded goal boxes. *First* choices were compared with chance by use of chi square; *total* choices were compared with chance by use of a *t-test*, after first normalizing the percentages by means of the arc sine angle technique (6). Post-training data do not include the first post-training block of trials since neither the morphine nor the food habit was clearly exhibited in these trials—probably a consequence of the delayed reward and/or the "place" learning involved in the training procedure.

RESULTS

Table I shows that both the morphine habit and the food habit were strongly manifested under their respective deprivation states, in terms

TABLE I

CHOICES OF REWARDED GOAL BOX

(Ratios: number of rats choosing rewarded goal over number choosing non-rewarded goal; percentages: mean proportion of 8 trials on which rats chose rewarded goal box)

	N	Morphine-habit rats		*N*	Food-habit rats	
		First choice	Total choices %		First choice	Total choices %
Pre-training	50	22/28 (n.s.)	48 (n.s.)	23	12/11 (n.s.)	47 (n.s.)
Post-training	26	22/4 (p .001)	71 (p .001)	23	19/4 (p .001)	78 (p .001)
2 hours after reward (morphine or food)	50	43/7 (p .001)	74 (p .001)	23	6/17 (p .05)	52 (n.s.)
2 hours after morphinized or fed in home cages	30	25/5 (p .01)	68 (p .001)	23	7/16 (p .07)	44 (n.s.)

of both *first* and *total* choices. However, after the rats had received their usual "rewards" (regular dose of morphine in the one case, regular feeding in the other), the two habits became dissimilar. The food habit was extinguished (in terms of *total* choices), and even inhibited as

indicated by *first* choices. In contrast to this the morphine habit was still exhibited in both *first* and *total* choices. These findings obtained whether rats were tested after receiving their "rewards" (food, or morphine) in the rewarded goal box or in their home cages. It thus appears that the food habit requires a drive in order to function, while the morphine habit is manifested in both the morphinized and morphine-deprived condition.

Experiment II

Method

After the above comparisons between the food habit and the morphine habit the food-habit rats were subjected to two further conditions and given choice trials as before. The question was whether an injection of morphine would prevent the extinction of the food habit which eating normally produced. The food habit was tested under the following two conditions. (1) Rats were injected with 5 mg./kg. morphine (their first experience of the drug); immediately run into the food goal box of the training apparatus and kept there for one half-hour, during which time they were fed their usual ration of mash; removed to their home cages for 1½ hours; then given 4 choice trials in the apparatus. This procedure was repeated 2 days later, with the regular food-training routine on the intervening day. (2) Rats were injected with 5 mg./kg. morphine; replaced in home cages and fed their regular ration of mash; then given 4 choice trials in the apparatus 2 hours later. Again the procedure was repeated 2 days later with the usual food-training routine on the intervening day.

Results

The results of these two tests are shown in Table II. It is clear that morphine counteracted the effect of food in extinguishing the food habit. This was unequivocally the case when rats experienced the action of the drug and ate their meal *in the food goal box* (food consumption was not noticeably different); that is, in terms of both *first* and *total* choices rats

TABLE II

Choices of Rewarded Goal Box by Food-Habit Rats

(Ratios: number of rats choosing the rewarded goal over number choosing the non-rewarded goal; percentages: mean proportion of 8 trials on which rats chose the rewarded goal box; *N*: 23)

	First choice	Total choices
2 hours after food reward	6/17 (p .05)	52% (n.s.)
2 hours after food plus morphine in food goal	17/6 (p .05)	72% p .001)
2 hours after fed in home cages	7/16 (p .07)	44% (n.s.)
2 hours after food plus morphine in home cages	13/10 (n.s.)	58% (p .05)

showed a decided preference for the food goal box. Under the other test condition, when rats were fed and morphinized *in their home cages*, their subsequent preference for the food goal box was less marked. Indeed, their *first* choices were essentially random. But in the course of four choice trials they did show some preference (p .05) for the food goal box. A comparison of *total* choices when rats were fed and morphinized in the food goal box with total choices when they were fed and morphinized in home cages revealed that the food habit was significantly stronger in the former case (t 2.368; p .05).

Discussion

The results of these experiments show that the morphine habit has properties different from those of the food habit. The latter appears to exemplify the laws of learning as specified by Hull (2, 7), with drive equivalent to a deprivation state, drive reduction mediating the learning of a habit, and the presence of a drive required to make the habit functional. The morphine habit violates these laws: it is functional whether the animal is deprived of the drug or freshly morphinized. In addition, where eating would normally de-activate the food habit, an injection of morphine results in the manifestation of that habit.

It is generally agreed that deprivation of morphine after a series of doses gives rise to a need or drive state in both man and animal, and that a further dose leads to drive reduction and thus constitutes a "reward" in man. Such drive reduction has been shown to reinforce learning in man (10), chimpanzee (8), and rat (1). On the other hand the present findings suggest that, at least according to Hull's (2) conceptual system, an injection of the drug acts to *produce* a drive state. Data from other experiments by the writer tend to substantiate this hypothesis. In one it was shown that 1 to 5 mg./kg. doses of morphine led to a significant increase in level of general activity in the two to three hours immediately following injection—as does the hunger or thirst drive. In another experiment it was found that a series of doses of morphine resulted in progressively and significantly increased water intake in the two to four hours following administration of the drug—as does induction of the thirst drive. Thus we arrive at a paradox: both morphine, on the one hand, and deprivation of morphine, on the other, would appear to produce drive. Moreover, it has been shown (1) that the continuing effects of morphine, the supposed euphoric effects, may lead to learning without the occurrence of drive reduction.

It is apparent that the molar systems postulated by psychologists who uphold the theory of drive reduction learning cannot explain the effects of morphine on learning and habit function. Hull's theory would make

a logical paradox of the findings. Moreover, it states explicitly that it is drive *reduction*, not *production* of a drive, which leads to learning. Some hope is offered by Skinner (**5**), who has proposed that certain drugs (for example, benzedrine) produce a state which multiplies the responses in the reflex reserve and may thus tend to prevent extinction of a habit—as morphine prevented extinction of the food habit in the present experiments. However, it is not clear how the drug state could lead to the acquisition of particular responses, that is, to learning.

It would appear that the concept of drive must be re-examined and re-stated so as to account for the facts. A lead in this direction is provided in the finding (Table II) that morphine was significantly more effective in re-activating the food habit if the drug was acting while subjects were having commerce with the stimuli which normally evoked the habit. This suggests that part of the reinforcing power of morphine may lie in some action on the sensory components of a habit. This possibility will be explored in a future paper.

REFERENCES

1. Beach, H. D. Morphine addiction in rats. *Canad. J. Psychol.*, 1957, **11**, 104–112.
2. Hull, C. L. *Principles of behavior*. New York: Appleton-Century-Crofts, 1943.
3. Montgomery, Kay C. The role of the exploratory drive in learning. *J. comp. physiol. Psychol.*, 1954, **47**, 60–64.
4. Sheffield, Fred D., & Roby, Thornton B. Reward value of a non-nutritive sweet taste. *J. comp. physiol. Psychol.*, 1950, **43**, 471–481.
5. Skinner, B. F. *The behavior of organisms*. New York: Appleton-Century, 1938.
6. Snedecor, George W. *Statistical methods*. Ames: Iowa State College Press, 1946.
7. Spence, Kenneth W. Theoretical interpretations of learning. In Stevens, S. S. (ed.), *Handbook of experimental psychology*, pp. 690–729. New York: Wiley, 1951.
8. Spragg, S. D. S. Morphine addiction in chimpanzees. *Comp. Psychol. Monogr.*, 1940, **15**, no. 7. Pp. 1–132.
9. Tatum, A. L., & Seevers, M. H. Experimental cocaine addiction. *J. Pharmacol. exp. Therap.*, 1929, **36**, 401–410.
10. Wikler, Abraham. A psychodynamic study of a patient during experimental self-regulated re-addiction to morphine. *Psychiat. Quart.*, 1952, **26**, 270–293.

BOOK REVIEWS

A Follow-up Study of War Neuroses. By N. Q. Brill and G. W. Beebe. Washington, D.C.: Veterans Administration Medical Monograph, 1955. Pp. xviii, 393.

The present volume is one of a series of VA Monographs on medical follow-up studies from World War II produced as a co-operative effort by the National Research Council, the Veterans Administration, and the U.S. Armed Forces. The work represents a tremendous advance in the systematic planning, analysis, and interpretation of psychiatric and sociological data over that shown in studies following World War I.

In contrast to the intensive study of a few cases in most clinical follow-up studies, this monograph reports the results of an extensive analysis of data collected by over 200 psychiatrists in follow-up interviews with a representative sample of 1,485 psychoneurotics, carefully drawn from the staggering total of 1.8 million U.S. servicemen with diagnosed psychiatric disorders. This follow-up, although important in itself, forms only one part of the over-all design. The study was planned to be an intensive inquiry into the "war neuroses"—one which would allow effective evaluation of screening criteria, assignment methods, and treatment and rehabilitative programmes.

Although positive relationships were established between several factors in the pre-service history and wartime breakdown or disability at follow-up, the hazards of psychiatric prediction are emphasized. The conclusion is reached that on the whole those who required psychiatric care in the services more than paid their way, and that the net effect of wartime breakdown was not as great at follow-up as expected. This suggests that too much emphasis has been placed on those factors thought to predispose an individual to breakdown. The growing realization that the manpower pool is not bottomless, and that the effective utilization of human resources requires a greater acceptance of marginal groups, suggests that greater emphasis, both in clinical practice and research, should be placed on those factors which *precipitate* breakdown. Understanding of the effects of various kinds and degrees of stress, for example, would permit a more positive programme of prevention through effective classification and assignment.

The study will be of interest to those concerned with the causative, therapeutic, and rehabilitative aspects of emotional disorders. It may prove to be an important historical document as well, since we all hope

that this is the last of the "war neuroses" studies. If the spectre of total mobilization looms again, however, this research will be invaluable to those responsible for the efficient utilization of manpower to meet the emergency.

J. M. Brown

Royal Canadian Air Force, Ottawa, Ontario

Culture, Psychiatry, and Human Values. By Marvin K. Opler. Springfield, Ill.: Charles C. Thomas [Toronto: The Ryerson Press], 1956. Pp. xiii, 242. $6.50.

In this book Opler outlines a proposal for the balanced development of theory and method in social psychiatry. At the outset he takes issue with the "grossly cultural" and the "rigidly psychogenic" approaches which have influenced personality theory to date. In place of these one-sided views he suggests a social psychiatry which incorporates varying cultural standards, since these are responsible for regional and group differentiation in mental illness, with the ontogenetic life-history approach of Meyer, Sullivan, and other neo-Freudians.

However, this combination of the cultural and the psychological can only be effective, in Opler's view, to the extent that value-theory is also considered. Here the author points out that both the social sciences and social psychiatry, as they moved in the direction of becoming generalizing behavioural sciences, have revealed a basic fear of dealing with "value-systematizations." Until this shyness is eliminated, Opler warns, these disciplines will continue their narrow view of man as "the prey of irrational and psychological forces."

The author's evaluation of the literature reveals the numerous failures, in present multi-disciplinary research, to observe the rules of scientific procedure in cause-effect analysis. According to Opler, these result in confusing over-generalizations, and also in over-emphasis of abstract "determinants."

In developing his point of view, Opler has brought together in a single volume the more important literature documenting the mental illnesses of the world's peoples. The compiling of these descriptive data alone would make it a book of merit. But, in addition to this, the author has produced a first-rate study, well organized and particularly rich in critical insight. The behavioural sciences will do well to give it serious consideration.

Robert C. Dailey

University of Toronto